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EDITED BY J. McKEEN CATTELL

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THE SCIENTIFIC MONTHLY

MARCH, 1923

THE CENTENARY OF GREGOR MENDEL AND OF FRANCIS GALTON[1]

MENDEL AND HIS CONTEMPORARIES

By Professor E. M. EAST

BUSSEY INSTITUTION, HARVARD UNIVERSITY

IN law the *death duty* is a tax imposed on the transfer of property at the owner's death. It is a tribute which the legatee pays to the public in return for an acknowledgment of title to his inheritance. To-day we publicly pay a death duty for the intellectural legacy which we as biologists have received from Mendel and his contemporary fellow-workers. We meet to proclaim our indebtedness to the men who found the study of heredity buried in the depths of a hazy mysticism, and left it one of the most firmly established branches of quantitative biology. There is an element of affection apart from the sense of justice in making this acknowledgment. We know that nothing that we can say here will add to or detract from the merits of these men of the latter part of the nineteenth century, but we wish to say a few words of homage as a free-will offering to the excellence of past greatness.

I was quite proud of the above paragraph when it was first written. Having listened attentively to several political orators on Memorial Sunday, I felt that it had just the proper shade of artistic solemnity expected on such occasions. In fact, it seemed as if both custom and decency demanded a series of eulogies attuned to a motive resembling the Dead March from "Saul." But I am told that this apparently decorous procedure is a mere subterfuge which deceives no one who has been taught the rudiments of interpreting the subconscious. What we really do in a case like this, I learn, is to use it as an opportunity for releasing various over-

[1] Papers in honor of the centenaries of the birth of Gregor Mendel and of Francis Galton, presented at a meeting of the American Society of Naturalists held at Boston, on December 29, 1922.

compensations from the springs of our own vanity. These supposed tributes are defense reactions. Their true purpose is to show how much better is the scholarship of the present day—that is to say, our own scholarship—than any which went before. Our motto is *De mortuis nil nisi bonum,* but our memorial wreaths are twined with poison ivy. We are not far from following the intent of the epitaph of the gentleman of the frontier, without a single redeeming quality, without a single friend, whose fellow-townsmen marked his last resting place with the truthful inscription: "In memory of John Black the citizens of Crimson Gulch are happy to erect this monument."

One is doomed, therefore; if he escapes Scylla, he is certain to be wrecked on Charybdis. According to my informant, if one praises, his encomiums are accounted like those of him who expatiates on the superb tennis of the opponent he has just beaten; if he is critical, he lacks the good taste to conceal either his envy or his egotism. It is disturbing thus to be initiated into the secret sordidness of the human soul. I had intended to utilize this auspicious occasion to inquire into the *reasons* for Gregor Mendel's successful masonry in laying a solid foundation for twentieth-century genetics. It appeared to be a worthy academic question which might have a modicum of importance in determining future trends in biology. I could have entered the affair stimulated by the valor of ignorance, like the Honorable William Jennings Bryan, and have said whatever seemed fitting; now I suppose I ought to be as circumspect as a speaker at a birth-control meeting with Anthony Comstock on the front seat.

Fortunately one need neither admit nor deny the contentions of these psycho-analysts. Let the actual personal motives for our actions be what they may, there is an obvious reason both for praising and for criticizing in this particular memorial celebration, which ought to be satisfactory to all concerned. Whether we belong to the aristocracy or the proletariat of science, whether our day be the nineteenth or the twentieth century, let us do one another the honor of believing that each can be counted among those who seek the plain unqualified truth. If, therefore, we do not judge those of the past as carefully as those of the present, we do both an injustice; in plain language, we are insincere ourselves, and we assume that our predecessors were such false scientists as to prefer hollow eulogies which lead nowhere to critical discussions which might aid in banishing error and fallacy.

When one speaks of co-mendelian genetic biology, he must include the sixty years previous to the beginning of the twentieth century if he is to obtain any enlightenment on the subject of Mendel's triumph. During this period there were three types of

work in progress which contributed directly to the establishment of genetics—experimental breeding, morphology and demographic mathematics. If one should undertake to enumerate the investigations which contributed their mites indirectly, he would be compelled to list every advance in knowledge made, for science seems to be like the colonial protozoan, faring best when cultivating interdependence.

The experimental breeding of the time was plant breeding. Animal breeding, of course, was much older; but as a method by which to discover new facts in pure science, it remained in the same stage of moribund quiescence from the time of the Babylonians, until rejuvenated by the quantities of interstitial tissue inserted by the zoologists of the present day. Plant breeding in England, France and Germany, on the other hand, had established several very interesting truths, such as the similarity of reciprocal crosses, the high variability of hybrids of the second generation when compared with that of the first, the dominance of characters and the reappearance of characters after being lost in the melting pot of the first hybrid generation. The names of the workers who made these discoveries are familiar. Among them are Gärtner, Godron, Lecoq, Herbert, Naudin, Vilmorin, Klotzsch, Carrière, Wichura, Hildebrand, Jordan, Haeckel, Henslow, Focke and Darwin; but with the exception of Darwin, I doubt whether any one of us could say who they were, what was their training, how they worked or what they added to the world's knowledge. Presumably they were all worthy men, who, outside of the ignominy of being botanists, had nothing to their discredit; yet if they can not be termed the unknown soldiers, at least they are the unremembered soldiers, of genetics. Why? Is the growth of science essentially so slow and so continuous that our attention is attracted only by a sudden showy change, which, like the bursting of a chrysalis, is merely the sequel to something of more importance which went before? Or, does a particular piece of work, such as that of Gregor Mendel—or rather Johann Mendel, to give him his correct name, have a value *per se* which transcends the others completely? Probably both questions should have affirmative answers. I think that all too often the unknown private deserves a considerable part of the credit usually given to the colonels and generals and chiefs of staff; but in this particular case, there is evidence of a real value to Mendel's contribution which puts it in a separate class. In this array of names are competent men, who worked hard and intelligently, who made discovery after discovery; but it would have made little difference to twentieth-century genetics if they had been tailors or bricklayers instead of plant-hybridizers. I think we need not lack in respect for every one of them if we say

frankly that they did not deliver the same class of goods. Mendel was familiar with the results of the earlier researches through Gärtner's huge compendium of investigations on plant hybrids, and he had read both Nägeli's paper on hybridization and Wichura's account of inheritance in the willows which appeared in the same year as his own work. There is no evidence of his knowledge of the investigations of the French horticulturists of the period, Vilmorin, Carrière, Godron and Naudin; but had he known them thoroughly, he would have been under no necessity of modifying the statement in the preface of his paper, wherein is shown more clearly perhaps than in any other place his grasp of the essential requirements of science. He says:

> Those who survey the work done in this department will arrive at the conviction that among all the numerous experiments made, not one has been carried out to such an extent and in such a way as to make it possible to determine the number of different forms under which the offspring of the hybrids appear, or to arrange these forms with certainty according to their separate generations, or definitely to ascertain their statistical relations.

Morphology, or rather the part of morphology concerned with cell development and in particular with germ-cell development, was not in the same case as experimental breeding. The cell theory had not only one great paper in the latter half of the nineteenth century—it had fifty, each of which gave real insight into the subject. In fact, by 1895, knowledge of the cell was almost as far advanced as it is to-day. It is doubtful whether even the facts which flooded from the pedigree-culture work between 1902 and 1912 were either so numerous or so valuable to general biological progress as were those discovered by cytologists between 1877 and 1887. The mere mention of names like Van Beneden, Carnoy, Fleming, Oskar Hertwig, Strasburger and Boveri, is sufficient to call to mind what master craftsman flourished in those days. No man could take *their* papers and point out that they had failed to avail themselves of the possibilities of the method of attack used. In so far as the method had possibilities, they were turned to account.

Of demographic mathematics, the third type of work useful to genetics directly, less can be said. On the purely mathematical side, the theory of probabilities, which, during the last two decades has been found to be of such great value in solving biological problems, had long been developed far beyond the immediate needs of biologists or their ability to apply it. Such application as it had throughout this period was largely as a means of grinding out various conclusions from human vital statistics; and the results were used by economists and by life insurance actuaries rather than by biologists. I doubt whether any attempt had been made to

apply the method to the solution of fundamental biological problems before the efforts of Mendel and Galton, although Quételet did use it in certain special anthropological researches.

This being the state of affairs at the time, can one by its consideration draw any helpful conclusions as to the degree of success or failure attending the efforts of the various workers involved? Such an attempt, I believe, is not altogether hopeless or wholly worthless. It is the type of introspection which every investigator ought to turn to now and then for the good of his own researches.

Let us assume the present stage of genetic thought to have been reached by a single extended and inclusive investigation, and that the technical requirements of this investigation can be determined by the simple biblical rule of judging the tree by its fruits. Having found these requirements, we can apply them as a yardstick to the actual investigations of the past and present. Without wishing to be dogmatic in the matter, I find them to be four in number. This is on the assumption that there will be no disagreement from the conclusion that science advances most rapidly by the use of the inductive method. They are: (1) the development of worthy laboratory methods, (2) the control of extraneous variables, (3) the determination of quantitative relations between the phenomena studied, and (4) the translation of the results into useful terms. The first three requirements are self-explanatory. By the fourth, I mean to say that science must have adequate shorthand formulæ by which extensive data can be expressed concisely, and that these formulæ must in turn be easily transformed into the everyday language of perception. Mendel's own system is a good illustration. Perhaps, like Archimedes, one also needs a fulcrum on which to rest his lever. It seems to me that many a piece of investigation fails to achieve the result which might well have been expected from its general conception and execution, because it begins in the middle of a complex problem and therefore has not the proper background of knowledge to carry it through. It tries to solve the cryptogram by taking a small sample from well toward the end. It usually works out much better to begin at the beginning, or at least where the other fellow left off.

Nothing novel is presented in this particular segregation of science essentials, and probably it is not so good a division as others could devise, but I believe that by keeping even this makeshift in mind, one can see rather clearly where the various contributions under discussion belong in the general scheme of things. It also gives one the opportunity of making a fair guess as to why Mendel's paper, which was in its way a model in form, remained with uncut pages for 35 years. He himself was fond of cheering his spirits by exclaiming "Meine Zeit wird schon kommen," but unfortunately it did not come until 16 years after his death.

Let us first endeavor to visualize the trend of thought during this period. There is first the host of hybridizers. Mendel took care of them in the sentence quoted. Without a proper background of facts, they had tried to solve these genetic problems having the greatest complexity. They had gone in for generic crosses by preference, and for species crosses by compulsion. Apparently it never entered their minds to make a cross between two nearly related varieties in order to simplify the complexity of the problem. They had the simple technique necessary for the particular mode of attack used; but they made no attempt either to eliminate controllable variables, to determine precisely the relation between the facts observed, or to reduce their discoveries to a system useful for predicting the consequences of like causes.

Turn now to the work of the cytologists. Naturally, in 1865, what we now accept as the fundamental facts were practically unknown. Only 15 years before botanists had been staging a battle royal on the subject of whether Schleiden's erroneous ideas regarding fertilization were true. The literature on the female gametophyte begins with Hofmeister in 1858. Real knowledge of the male gametophyte dates from Strasburger's paper on cell-formation and cell-division in 1877. The actual cells concerned in the fertilization of the higher plants were not described clearly until 1884. And the zoologists were no more clairvoyant than the botanists. Virchow did not develop the theory of cell continuity until 1858, and his papers, as is usual with new ideas, provoked attempts at scientific sabotage for a decade after that time. Fertilization of the egg by one spermatozoon awaited the demonstration made by Oskar Hertwig in 1875. The identification of the cell nucleus as the most important vehicle of inheritance, though suggested by Haeckel in 1866, was not made until it was emphasized to the world by the independent investigations of Strasburger, Hertwig, Kölliker and Weismann in 1884; and the reduction division of the chromosomes was not shown clearly until the appearance of Boveri's work on Ascaris in 1887. The general constancy of chromosome number, their individuality in size and shape, and the details of their behavior during maturation and fertilization did not come until well within the limits of the present generation.

But one must not be blinded by this course of events. Mendel's paper shows a clear grasp of the gross facts of fertilization, and those gross facts were sufficient for his needs. Furthermore, though, the cytological details which would have made it easy for others to grasp the full significance of the paper were not available until the second decade after publication, it must be remembered that recognition of his work did not come until 15 years after these

details were common knowledge. For these reasons I can not believe that it is correct to account for the peculiar neglect of Mendel's work by assuming it to be ahead of the biological *knowledge* of the time.

Let us look a little closer at the problem. Cytology has been said to be the statics, and controlled breeding the dynamics, of genetics. Perhaps there is enough truth in this analogy to show a slight difference in point of view, but even this is doubtful: they are both statical in nature, and differ most in the limitations imposed by the mode of attack. For example, if one were to take all of the facts discovered by pedigree culture work, he could infer a certain organization and mechanics of operation in the germ-cells; if, on the other hand, one were to correlate properly all the discoveries of cytology, he could draw rather accurate conclusions regarding the actual transmission of characters. Genetics could not have developed as it has without both points of view, however, for *a priori* possibilities, unless tested, have no essential value. But granting this to be the truth, even the most ardent cytologist will admit that from the broad point of view of general genetics, his calling has its defects. It has a beautiful technique; but the very fact that the laboratory methods are so refined makes it difficult to eliminate the obscuring influence of extraneous variables by the very commonplace contrivance of investigating large quantities of material under controlled conditions. Again, it is practically impossible, due to the nature of the method, to investigate material in such a way as to obtain an adequate statistical representation of the facts which will permit verifiable predictions to be made. One does not decry cytology in making this statement. Cytology deserves the highest respect. But it is necessary to point out the inherent difficulties under which the cytologist works, difficulties which make his accomplishments so much more to be acclaimed. He was in much the same predicament which one might imagine would be the plight of a group of scholarly Martians who found a stranded aeroplane out of gasoline. Using the best method under the circumstances—the cytological method—they would dissect the strange visitor carefully, and make the most minutely accurate drawings of the various parts. They would then speculate on the use of each part, and finally form a hypothesis on the value and use of the apparition as a whole. This procedure would be perfectly proper. Without it, they would probably be in a quandary to know what to do with the can of gasoline dropped overboard by the unfortunate birdman and finally found by a young Martian piscatorial expert at the bottom of a canal ten miles away. With it, the gasoline would be poured in the tank, and the hypothesis

tested forthwith. Now my belief is that Mendel found the can of gasoline and by his own method of reasoning knew what to do with it. But after stealing in at night and making the apparatus run, his fellow-countrymen were not able to understand his account of the machine, because the method of dealing with it was so foreign to their own experience.

I am speaking absolutely seriously. It will not do to attribute the plight of Mendel's researches to the limited circulation of the Brünn journal, for it was received by all the various universities of Germany and by many foreign libraries. It was due to a different cause, the inability of the biological mind to adapt itself quickly to an experience it had not had before. Mendel was not really a biologist though he investigated the heredity of both plants and animals—you will remember that he worked with bees as well as peas, though the records of his experiments with the honey-makers have never been found. Biology was one of his numerous avocations, like playing chess, organizing fire brigades, running banks and fighting government taxes. He was really a physicist, and brought to one of the great problems of biology the attitude of mind and the quantitative method of attack which had been in use for some time by physicists and by astronomers, and which was just coming to be used more widely by chemists. It was an unknown language to biology, though it fulfilled the essential requirements of scientific research better than anything which had gone before; and it came to biology at a time when those who were endeavoring to investigate inheritance by means of hybridization were not prepared for their task, and thirty years before the results of the slow-going cytological method of attack had progressed so far as to permit the formulation of a well-rounded hypothesis near enough to the truth to make it possible to outline the points to be verified and to make recognizable a plan of verification. Great as was the advance in cytological genetics during the latter half of the nineteenth century one can not imagine an appreciation of the Mendelian type of work by any of the investigators. Their minds were too carefully focussed on the individual fact. Either Darwin or Galton would have seen the truth clearly; but then Darwin and Galton were amateurs who were not trammeled by professional connection with the guild of biologists.

One finds additional reasons for accepting the point of view that it was the *method* which made Mendel's paper great, and the *novelty* of the method which made it unappreciated, if he studies carefully the generalized hypotheses on the subject of heredity during the nineteenth century. Really one does not need further demonstration if he has followed genetics from the time it passed out of the larval stage in 1900, and has seen how many of us as-

sume we are exhibiting a fine degree of super-scientific criticism instead of mere stupidity when we adopt the agnostic attitude toward novel genetic methods and newly discovered facts. But the pernicious influence of abstract theories on the mind, the seductive way in which such theories lead away from reality, is worthy of a word on its own account.

Paucity of facts did not prevent the author of yesterday from putting forth theories of heredity by the score. One has only to examine some of the huge tomes on heredity of 30 or 40 years ago—Nägeli's "Abstammungslehre" is a good instance—to realize that lack of knowledge was even an aid to the publishing business. Nägeli was able to write whole chapters on certain subjects manifestly because he had absolutely no information on them.

Each and all, these theories were as one in using mechanical interpretations which postulated active ultra-microscopic living units endowed by their creators with various qualities. No doubt this was just and proper. Such concepts have been found useful in various branches of science, and have been retained in the current theories of heredity. I only wish to point out that many of the hypotheses, described in such fanciful detail, have been hindrances rather than helps. If each of us were asked privately to state the object of scientific hypotheses, we should probably say, "to help formulate tests by which various assumptions can be justified or refuted." But publicly, in the class-room, and in the journal, we are very likely to become enamored of a well-presented hypothesis which does not stimulate research a whit, just because we are beguiled by its plausibility. Certainly a great many of the points discussed at great length by these early geneticists do not fit any better into a scientific discussion than would Cotton Mather's disquisition on the number of angels who could deploy on the point of a pin.

Recall Darwin's provisional hypothesis of pangenesis, as he termed it, proposed in 1868. His units were the gemmules, which were being given off constantly by every cell, including the germ-cells. That was virtually all there was to it, though it was propounded as a theory of heredity. In reality it was a prop, and a very weak one, to the theory of evolution. Darwin postulated this brisk inter-cellular trade in gemmules in order to show the literal-souled biologist how acquired characters might be transmitted. On the assumption that somatic modifications are not inherited, it was unnecessary. With the latter view, the cells might just as well have been insulated from each other as thoroughly as the wires in a telephone cable. Though it be sacrilege to say it, this was not the type of production to be expected from the author of the "Origin of Species." Apparently it stimulated but one ex-

periment, Galton's blood transfusion experiment, which we now know could have told him nothing one way or the other. It acted rather like Aaron's rod, with ink gushing out in place of water, and this effect was not for the good of science.

The most notable among the various modifications of this type of theory was that of DeVries, published in 1889. Here the corpuscles, which he called the pangens, represented potential elementary body characters rather than cell qualities, and the universe of their activity was the cell rather than the body. DeVries's theory, perhaps, was some small philosophical advance over that of Darwin, but neither was a real working hypothesis in which the possible mode of hereditary transmission was outlined in such a manner that the biological student was led to make experimental tests of the postulates involved. No doubt general evolutionary thought was somewhat clarified by their introduction into the literature of the day, but they stimulated words rather than work.

Nägeli's "Abstammungslehre," which appeared in 1884, has been credited with being the first theory of heredity endowed with qualities calculated to induce research. But was it such a theory? Nägeli proposed to distinguish two kinds of protoplasm built up of physiological units, the micellæ; the one was wholly nutritive in function and required no special architecture; the other, the idioplasm, was a structure of elaborate constitution built up from micellæ representing the potential characters of the organism. I can find nothing more in Nägeli's work, and it took him 822 pages to say this. Here was a man to whom Mendel had written in detail about his work during the years between 1866 and 1873, a man who had contributed notable papers to botany on subjects ranging from the form of the starch grain to hybridization, a man who discoursed at such length on chemistry and physics that one might suppose him to have had the greatest sympathy for the highest type of useful quantitative work; but he devotes absolutely no time or energy to discussing the one paper which might have shown him a way out of the wilderness in which he found himself. Was this science? I do not believe it was. It may have been only the garrulity of senility, it may have been philosophy, but it certainly was not science. There is no evidence whatever that it stimulated a single investigation or was the source of a single discovery. But Nägeli was no worse than the other theorists of his time. The three hypotheses mentioned are fair samples of some twenty or thirty which were proclaimed to the world during the last half of the nineteenth century. They have been cited only because they show, as nothing less concrete will show, where the unreal leads.

The obverse of the medal can be illustrated by Weismann's presentation of the subject.

In Weismann's theory, the idioplasm, or germplasm, was identified with the chromatin of the nucleus. The ultimate living unit, the biophore, was a kind of biological atom active in building up organic characters. They grouped themselves together into determinates which controlled the specialization of cells. The various determinants of an organism made up the ids contributed by past generations. The ids might be one or many; and where more than one might differ slightly among themselves, thus providing for variation within a species. The ids formed the chromosomes or idants by arrangement in a linear series.

These postulates seem simple enough and not unlike those of earlier theories, but the way Weismann reasoned in endowing his corpuscles with qualities was a distinct advance. It was made possible by his thorough knowledge of embryology in which he had previously made notable contributions.

Denying the inheritance of acquired characters, and doing much toward demolishing the fallacious logic put forth as proof at that time by adherents in the belief, Weismann outlined a very stimulating conception of heredity on this basis. The immortal germplasm was assumed to be set apart at a very early cell division and passed along unchanged to the next generation, except as the activities of the living units produced occasional changes in its constitution. A provision for accurate equational division of the chromosomes and their reduction in number at the maturation of the germ cells was thus demanded, predicted and afterward realized—though not precisely in the way he supposed—by discoveries in the field of cytology.

Weismann further accounted in part for evolution by a selective struggle between the determinants of the germ cells, and for individual development by a qualitative distribution of the determinants of those cells set apart to build up the bodies which were to act as hostelries for the immortal germplasm.

No matter what views one holds as to the precise amount of truth contained in Weismann's generalization, it is obvious that it is very different from the others mentioned. Many geneticists believe the modern theory to be the outgrowth of Weismann's ideas. Wilson says he brought "the cell theory and the evolution theory into organic connection." Morgan credits him with the basis of the present attempt to interpret heredity in terms of the cell, in that he suggested three of the principles used in this interpretation. Be this as it may, there is no doubt but that Weismann was the first to utilize all the facts at his command, and to utilize them very ingeniously, in building up a theory of heredity, which, whether true or not, had numerous points that could be tested by experiment. In my opinion, it is by this criterion of ultimate usefulness and not

by any analysis of its content of reality, that its greatness should be measured.

This presentation of nineteenth century genetic work necessarily having been very sketchy, no apology need be made for summing up the points of the thesis involved. Mendel initiated a method whereby the elementary quantitative relationship between hereditary phenomena could be tested and retested, and expressed his results in an algebraic notation of greatest usefulness. He thus supplied to genetics an essential methodological requirement which it previously had lacked. The significance of his offering is now apparent; but the history of both genetic research and genetic theory show that biology was not ready for such a profound change at the time. The investigators were satisfied with defective methods because they were yielding important results, and were capable of continuing to yield important results up to a certain point; and those who theorized, not realizing the defects of the current methods of research, wandered about aimlessly in the universe of the unreal. Obviously, if one is to find a clear exposition of genetic thought anywhere, it should be in the generalized theories of heredity. Taking them in the order of their issue, there ought to be a history of the development of this thought. And it seems to me that they show clearly that previous to 1890, biology was unprepared for the quantitative method of physics and chemistry; yet this method was a prerequisite for continued progress. Let us put the matter in another way, for the sake of emphasis. The older methods of genetic research were inadequate, the breeding work because the workers did not know how to use their tools, and the cytological work because the workers lacked the proper tools. Nevertheless, they made progress. They built slowly but firmly an edifice that future generations may well admire, much as the laborers of ancient Egypt built the pyramids. Finally, there arrived the point when a man like Weismann could piece together a well-rounded theory of heredity based almost wholly on this cytological evidence, which was testable by experiment. But for the tests required a new method was necessary, and this method was not forthcoming until the discovery of Mendel's long-forgotten paper. From that time onward, genetics entered a new era.

Properly, this paper should come to an end at this point; but I can not stop without delaying a moment to pay a passing tribute to Francis Galton, even though I realize that Dr. Harris will do full justice to his memory. Galton, as mentioned before, was one of the few of Mendel's contemporaries who would have appreciated his work. He was a kindred soul to Mendel, a brilliant amateur, interested in everything; and but for a matter of mere chance, he probably would have reached the same goal. The matter of chance

was the study of ancestors instead of descendants. It seems a minor point, but it turned out to be important. Thus the Fates play with mankind. Galton was a leader of thought in England; he was no novice in biology; his capacity in mathematics was unquestionably great; and he turned instinctively to experiment. If only this single slender thread had not obstructed his efforts, one can well imagine how far he would have gone. But perhaps it is all for the best. Statistical theory needed Galton's guiding hand. It would not be the same to-day had it not received the quickening touch of his genius. Peace be unto his name!

THE BEARING OF MENDELISM ON THE ORIGIN OF SPECIES

By Professor T. H. MORGAN

COLUMBIA UNIVERSITY

STUDENTS of genetics have often been challenged to state the bearing of their work on the old controversy about the origin of species. If the challenge has often been allowed to go unanswered, it is not because geneticists failed to have an inkling that their results might, in the end, have a significant bearing on this question, but rather because they recognized the need of first putting their house in order. The futility of attempting to arrive at any reliable conclusion concerning the origin of new types until something more was known about heredity had become only too manifest during the debates of the latter half of the last century. It required no subtlety on the part of geneticists to see that only those characters can take part in the process of evolution that are inherited. It seemed to follow that it would be better to find out what characters are inherited and how they are inherited before the controversy could be continued profitably.

Geneticists do not make any claim to have solved the problem of the "origin of species." I am afraid to "protest too much," for fear that you may conclude that we really do think so. We can (and we know that we can) furnish certain evidence—important evidence, we believe—that bears on the origin and mode of inheritance of new types. It is this evidence that I am going to consider to-day. How far these new types furnish the variations that make new species may depend on what we call "species." If, as some systematists frankly state, species are arbitrary collections of individuals assembled for the purposes of classification; or if, as other systematists admit, there are all kinds of species both in nature and in books, it would be absurd for us to pretend to be

able to say how such arbitrary groups have *arisen*. It is possible that some of them may not have arisen at all—they may have only been brought together by taxonomists.

I am not criticizing the taxonomists, but I am letting you know that I know that we are embarked for the next quarter of an hour on a hazardous undertaking.

I am not sure, moreover, how far students of taxonomy want our help. They suspect us a little, bearing gifts. I am not sure just what they could make of our conclusions if they accepted them. The systematist may be quite right in following his own methods of arranging the animals and plants living on the globe to-day; he may be quite content to allow the geneticists to make a different arrangement. Neither is quite decided at present whether or not to let the other alone. However, there should be no quarrel between us! On the contrary, I have never failed to find that we have innumerable points of contact. I am of the opinion that we can be of mutual assistance, and I sincerely hope the systematists present will agree in this pious wish.

The *modus vivendi* that I suggested a moment ago seems to me to have its points. Is it not possible that the kind of classification the taxonomist needs for purposes of identification may be very different from the classification that the evolutionist needs to indicate lines of descent or of relationship? Is it not possible that the geneticist may need still another classification to indicate how many genes certain types have in common and in how many they differ? Each of us might, if he wished, erect a species definition of his own, and each would be within his rights in forming such a definition. Whether it would be desirable for the evolutionist to use the word "species," that tradition has assigned to the systematist, is a question for the evolutionist to decide; but, as I have said, it is a perilous adventure for a geneticist to attempt to interpret the historical species in terms of genes. It may also be a work of supererogation.

Hence, whenever I refer to species, in what I am about to say, it is very probable that I shall sometimes use the word in all its vague implications—much in the same sense in which Darwin used it; but when sharply defined issues are at stake I shall try to remember to use other words.

Animals and Plants under Domestication

Darwin's largest single contribution to the origin of species grew out of his observations and experiments on "Animals and plants under domestication." Here also is the field in which modern genetics has reaped an abundant harvest. Taken broadly

the results have strengthened Darwin's thesis that by artificial selection man has brought about those adaptations to his needs, or to his fancy, that our domesticated products show. Granting that some of the variety shown by cultivated plants and domesticated animals has also been obtained by outcrossing with different wild races, there still remains a good deal that appears to have arisen by the selection of new mutant characters that have appeared under domestication. It is seldom possible to tell whether a variation was obtained by outcrossing, or by mutation; but, if, as I shall try to show, the heritable differences that distinguish wild types and races also represent mutant changes in the germ-material, then it makes practically little difference whether new characters arise in nature (and are later incrossed), or under domestication (and then inbred).

The evidence that all heritable variations may have had the same kind of origin rests on the following facts and argument: We have found that the mutant types that appear in our cultures follow Mendel's laws of inheritance; practically all the character differences of domesticated races also fall under these same laws; it can scarcely be questioned, therefore, that we are dealing in both cases with mutant characters.

There are also records of mutants appearing under nature that have been found to follow Mendel's laws. There are also cases in which wild varieties, differing from each other in distinct characters, have been shown, when crossed, to come under the same laws.

This accumulated evidence speaks strongly in favor of mutants as furnishing the basis for artificial selection, regardless as to whether the mutants have appeared under cultivation or in nature.

Darwin knew about mutants, calling them sports; and, as every one knows, he rejected sports as furnishing the kind of steps that the evolution of species seemed to call for; because, he said, such gross modifications of particular parts of the body as are seen in sports could rarely be adaptive. Only by small changes in a great many parts could arise those interrelations of parts necessary for survival.

To-day we agree with Darwin that such extreme variations as those he called sports would rarely, if ever, have contributed to the formation of new types in nature. But we also know that minute differences also arise as mutants, and that these are inherited in the same way as are the larger mutant changes. It is also now clear that these smaller mutant variations must be those small heritable variations that Darwin himself appealed to as furnishing the materials for organic evolution. In these respects we have made great advances in knowledge since Darwin wrote; and I

doubt if a single geneticist familiar with the evidence at first hand will hesitate to make this substitution. We have learned to distinguish between those individual differences due to the environment (that are not inherited) and those that arise as mutations (that are inherited). Superficially there is no way of telling one from the other, since they overlap and involve the same changes in the same characters. But by pedigree work the essential difference can be made evident, as Johannsen demonstrated in 1909.

What was not entirely clear, when Darwin wrote, has been set straight. This is one of the most notable advances in the study of variation since the publication of the "Origin of Species."

Multiple Effects of Single Changes in the Germinal-material

We have also discovered another most significant fact about those changes in the germinal material that produce mutant characters. It has been found that a single change in one gene often affects the animal or plant in more than one way; sometimes in many parts of the body. Even very different kinds of organs are often affected by the single change. Students of genetics have known for some time that the so-called unit character is a fiction—one that may have been excusable in the earlier stages of the work, but one no longer tenable or desirable. To-day we are familiar with many cases that show the multiple effects of a single change in the germ-material.

It is true that we still find it convenient to single out that effect of the gene that is sharpest, most easily observed and most convenient in the separation of Mendelian classes. We often visualize this particular effect as the single result of an alteration in the germ-material; but no practical geneticist forgets that as a rule many other effects are also produced by the same mutant change. DeVries laid emphasis on this point. He regarded each mutant change as one that affects the individual in every part—made a new elementary species out of it, he said. I think de Vries' view is much nearer to what we actually find, when mutants appear, than is the view that over-emphasizes unit-characters.

If, then, as I have just said, we pick out a superficial effect of the mutant change, as the symbol of that change, we do so because we can most easily follow its course in heredity. We ignore as a rule other subsidiary changes in the organism, such as those involving physiological processes; but the literature is full of incidental references to such subsidiary effects. Pearl's recent studies of the length of life of mutant races open up a new field of investigation in which the physiological by-effects of superficial mutant characters probably play an important rôle. It need not be argued, I suppose, that slight changes in the physiological effects

of a character of a species are those that most nearly affect its chances for survival. I think it would be rational to take for granted that changes of this sort have been the ones that have played the most important rôle in evolution. Now reverse the argument! If beneficial mutant changes, involving physiological changes, also often affect superficial parts, it is the latter, being visible, that might be chosen as the mark of the species. Here we may find an answer, I think, to the old riddle, that while natural selection is supposed to produce new species by the selection of variations essential to the life of the species, our definitions of species are based almost always on trivial, superficial characters that have, so far as known, no survival value.

In other words, the systematist has followed the same course as the geneticist. He has chosen superficial differences as the distinguishing marks of his species—he has not been concerned with the characters that have in reality created the species.

If, then, physiological changes have most often been the basis of natural selection, it follows that we may get into an inextricable tangle, if, taking the systematist's definition of species, we attempt to harmonize such a definition with physiological differences between species to which the taxonomic definition has only a secondary relation. I am inclined to think that a good deal of unnecessary worry can be traced to this source.

Losses of Characters and Absences in the Germinal Materials

Within the ranks of geneticists themselves doubts have sometimes been expressed as to whether *any*, even the smallest, of the mutational changes that we study are of such a kind that they could produce the advances in complexity that evolution is supposed to demand. I shall not try to avoid this issue by pointing out that evolution is also sometimes backward as we say, *i. e.*, towards simplification. It may be conceded at once that many, perhaps nearly all, of the mutant types, that appear in our cultures, show not only deficiencies and losses of characters, but even that most of them could not possibly have any significance for progressive evolution. These admissions do not exhaust the subject by a long shot.

Let us look a little deeper into the situation. No one doubts that each animal and plant is adjusted in a great number of ways to the complex environment in which it lives. We can imagine hundreds of changes in any animal, but it is difficult to suggest one that would certainly be an improvement, when all the many sides of its existence are taken into account. Is it not clear, then, that almost every random change must be a disadvantageous one? This is what we actually observe when a new modification of an old character takes place. But note! Among the multifarious possible

changes there *may be one* that is an improvement, in the sense that the new animal is better adapted to the old environment, or that it can better adjust itself to a slightly different one. This possibility suffices for natural selection.

In the limited range of our personal experience it is not to be expected that the mutations we find would be advantageous ones, but when we consider the vast number of individuals that make up a species, the supposed difficulty does not appear insuperable.

Bateson, who has emphasized the fact that most mutant changes are losses and deficiencies, draws the conclusion that loss in the character means loss also in the germ-material. If it were true, as he supposes, that loss of character is to be interpreted to mean that something has also dropped out of the germ-material, then I think we might begin to look elsewhere for the materials of evolution, for I can not follow Bateson in his suggestion that evolution may only mean a succession of losses. I believe the premises are wrong.[1] Again, when we look at this question of losses in character from the point of view of embryology, it is not in the least surprising that almost *any* kind of change in the germ-material would bring about defects in character. If each character is the end result of a long series of developmental (embryonic) stages, it follows that almost any alteration at the start will be expected to make less perfect the end result, for less perfect here means only something different. This I take it is what most often happens.

It also seems to me quite illogical to infer that because a change in the germ-material may bring about a defect in the developmental process, this change in the character is to be interpreted as a loss from the germ-material. Such a conclusion seems not only unnecessary, but, what is more important, it is in flat contradiction with the only critical evidence that we have bearing on this question. I mean the evidence from the order of appearance of the multiple allelomorphs of Drosophila.

Much also has been said concerning dominance and recessiveness of mutant characters in relation to the characters of the wild species. Bateson has emphasized the fact that most mutants behave as recessives to wild type characters. Hence he concludes that these

[1] In his Australian address in 1914 Bateson's purpose was to point out to what conclusion one is led on the assumption that mutations are losses and if mutants are assumed to furnish the materials for evolution. In his Toronto address in 1921, on the other hand, Bateson appears to argue that the distinctive differences between wild species are something added, and therefore not the kind of variation about which genetics concerns itself.

The argument advanced here in the text accepts neither alternative, but rests on the interpretation that a mutation need not represent a loss of germinal material, and that the differences between species are probably mutant differences.

mutants do not seem to be the stuff from which wild species are made. Now, in the first place, while the statement that nearly all mutants are recessives may hold in certain cases such as Drosophila, yet there are other groups in which the number of dominant mutant characters is much in evidence. But the distinction itself between dominant and recessive is by no means so general as implied. Mendel described only nine pairs of characters, choosing those in which dominance is complete. To-day, we look upon these as rather extreme cases, not as the typical ones. We meet with many cases in which dominance is imperfect. The hybrid is intermediate between the two parental types, especially when all the different modifications of the pair of genes in question are taken into account. The same gene may, in fact, be dominant in one respect and recessive in another. When the hybrid is intermediate, it is often purely conventional which alternative is chosen as the dominant. The real point at issue—Mendel's great discovery—is that genes separate cleanly in the germ-cells, even when the hybrid is intermediate. It befogs our problem, I think, to insist that dominance means complete dominance. With this sharp distinction done away with, the difficulty loses much of its apparent point.

Infertility between Species and Sterility of the Species-Hybrid

One of the oldest questions concerning the origin of species by the summation of individual differences is this—how has the infertility, commonly observed when species are crossed, arisen? No incipient infertility, it is said, can be observed when different breeds of domesticated animals are crossed. Darwin had to face this question, and met it in the only possible way that it could be met at that time. He pointed out that there is in reality no such sharp distinction as implied. He showed in a large number of cases that well-recognized species do cross, and that sometimes they even produce fertile offspring.

Since Darwin's time a great deal of work has been done by embryologists that bears on this relation. Every embryologist is familiar with the fact that sea urchins belonging to different genera and families can be cross fertilized. The early stages of development of the hybrids are often normal. It is only when the conflicting processes that are induced by the inherited characters of the egg and sperm begin to crop up that difficulties set in. When we take into account the delicately balanced processes that each stage in embryonic development involves, it is not in the least surprising to find an incompatible situation when conflicting interests are brought together.

Then again, in the higher plants mechanical and physical dif-

ferences may often account for the failure of the foreign pollen tube to reach the egg. Similarly, in matings between two species of animals there may be incompatible structures or responses. When such matters as these are given sufficient weight it is not going too far to claim that we are not dealing here with a single fundamental difference, but rather with several different kinds of processes that give like results.

It is true that new mutant types are fertile (if fertile at all) with the type from which they arise, because the single difference that distinguishes one from the other is a compatible difference. If it were not, the new type would be lethal. When, however, two types have been separated for a sufficiently long time, differences may be supposed to arise in one, or in both, that are incompatible in fertilization or in development. This seems to me to cover the case from a theoretical point of view.

Even the infertility often observed between the pollen and the ovules of the same plant is now in a fair way of being explained. The recent work of Correns, of East and of Compton has shown that such kinds of infertility depend on the presence of one, or at most a few, genetic differences. In such cases the failure to fertilize appears to rest on differences in the somatic tissue and not on incompatibility of sperm and egg or on difficulties in embryonic development.

That infertility may arise as a consequence of genetic differences has been shown by the recent work of Jones on corn and tomatoes. When pollen from one race was placed in competition with pollen of other races that differed only in minor features it was found that the plant's own pollen was the more efficient, *i. e.*, it fertilized proportionately more ovules. The result may fairly be interpreted as a case of *incipient* infertility in outcrosses. We do not know how frequently such a relation exists, because the problem has been very little studied with critical standards. The essential conditions for such work are seldom realized. On the whole, then, is it not a little remarkable to find in the one case where the problem has been adequately examined that the outcome has been positive?

Bateson has recently laid much emphasis on the *sterility of the hybrids* themselves in species-crosses as compared with the absence of such sterility when mutant races are crossed. In the latter case not only is there no incipient sterility in the F_1 offspring, but on the contrary the F_1 heterozygotes may, and often do, produce more offspring than do individuals of either parent race.

Bateson has laid a great deal of emphasis on the importance of this question for modern genetics. His studied wording of the requirements that would be necessary to demonstrate that inter-

racial sterility had arisen in connection with mutation is worth careful consideration: "The production of an indubitably sterile hybrid from completely fertile parents, which have arisen under critical observation from a common origin"—this "is the event for which we wait."

Bateson has made the conditions of demonstration extremely difficult. He postulates that "the event for which we wait" is one that must suddenly occur—as a mutation perhaps? If so, and if as seems to be probable in the case of other mutations, the expected change should occur in only one gene, it would by definition make sterile the animal that received this gene, and hence defeat any effort to prove its fertility. There are possible ways of escaping this dilemma, but these would detract from the generality of such origin of sterility.

Suppose, in order to avoid the contradiction in terms just referred to, we ignore the probability that mutations take place in one gene at a time, and suppose that a single individual of the new type called for has appeared as a duplex mutant. The chances are very great that it would be lost before its value was appreciated for the simple reason that it would have to be crossed to the original from which it arose in order to get any offspring at all. It is with this type that it is by hypothesis expected to give sterile offspring. Only when several individuals of the new imaginary type arose at once, or as a bud sport, could a race be produced with which to properly test the question of sterility of mutant hybrids according to definition. These and other considerations raise the question as to whether the sterility of species hybrids may be of such a nature that we are justified in insisting that they must arise under the conditions that Bateson postulates as essential. There is at any rate another side to the question which may throw some light on the situation from an entirely different angle. It is now well known that in those stages in the development of the germ-cells, that are concerned with the conjugation of the chromosomes, irregularities occur in the distribution of the chromosomes in species hybrids. As a consequence many or even all of the germ-cells are abnormal. Hence arises the sterility observed in such hybrids. In many cases these irregularities seem to be connected with differences in the parental numbers of chromosomes. Such a situation would not be expected to arise when mutation in a single gene has made one parent different from the other, but might be expected, if, due to doubling of the chromosomes of one of the parental gametes, there is produced a triploid individual. In fact, a disturbance, similar to that in species hybrids, has been described in some at least of the mutational triploids that have been examined. It would, however, be a mistake, I think, to assume that

the sterility of all species hybrids is due to differences in the parental chromosome numbers, although this may be the expectation when the numbers are different. There are other cases where the parental chromosomes are the same in number and the species hybrid is sterile; hence, there may be still other kinds of differences that make conjugation of the chromosomes difficult or impossible. One of the events for which I wait is the demonstration of such differences that interfere with the conjugation of the chromosomes and tend in consequence to produce sterility.

Recurrent and Parallel Mutants

Finally, the demonstration that the same mutant types recur over and over again has opened up new points of view with interesting consequences. It has long been known, in a general way, that the same kind of mutants reappear *in the same species*. We are now beginning to get evidence from pedigree cultures that the same types may occur *in different species*. At present there are only two ways in which we can be sure that the latter are due to the same kind of change in each species. One way to prove this is by crossing the two mutants. If both mutant characteristics are recessive, and give the recessive when crossed, the proof is established that they are identical mutants. Such a case has arisen between the two species of Drosophila simulans and melanogaster. Sturtevant has shown that there are thirteen mutants that are the same in both species.

The other way of showing that two mutants arising in different species are identical (isomorphs) is to find their linkage relations with respect to other mutants that also appear to be identical in the two species. This involves the possession of several such types in both species, as well as a fairly complete knowledge of linkage groups in both. It may take several years before enough material can be brought together for a safe conclusion, but the outlook is promising.

If, then, it can be established beyond dispute that similarity or even identity of the same character in different species is not always to be interpreted to mean that both have arisen from a common ancestor, the whole argument from comparative anatomy built upon the descent theory seems to tumble in ruins. This, however, is only a first impression; for, even if it be true that some of the resemblances between species may be due to identical mutational changes in the same gene, there remains the vast array of other characters that the two species still retain in common. These furnish the hint that the evolutionist needs to make probable his theory.

Should it turn out to be true that a large number of similarities

in species are due to similar mutations in the same gene, then, in future, the student of genetics will be more interested in detecting these identities than in taking account of the genes that have not yet produced new mutants. The evolutionist will be concerned with the genes that still remain unchanged because these will indicate a common ancestry. But, I think, he will be at his wit's ends to exclude from his lists those similarities that are due to identical mutational changes. Lest you infer that I am letting this idea run away with me, I should like to add that we are also only too familiar with cases where mutations, in quite different genes, produce effects that are so much alike that it takes a microscope to tell them apart—and even this may not suffice. Sometimes we even have to appeal to statistics to help us out.

Nevertheless, the discovery that the same mutation happens over and over again, not only within the same species but in different species, is, I think, one of the most interesting discoveries in recent genetic work. It means that certain kinds of changes in the germ material are more likely to occur than are others. If we adopt the Galton metaphor of the equilibrium polygon, these changes might be interpreted to be the more stable conditions of the genes. Or, if we prefer to think of the change in the gene as a chemical event we can form a somewhat different picture to ourselves as to what happens. Whatsoever way we prefer to symbolize the recurrence of the same event in the same gene the significant feature remains—the appearance of new variations in the hereditary material is something less a random process than we had hitherto supposed.

GALTON AND MENDEL: THEIR CONTRIBUTION TO GENETICS AND THEIR INFLUENCE ON BIOLOGY

By Dr. J. ARTHUR HARRIS

STATION FOR EXPERIMENTAL EVOLUTION, COLD SPRING HARBOR, LONG ISLAND

I

FRANCIS GALTON and Gregor Mendel have much more in common than the mere incident of the identity of the years of their birth. Both men worked in advance of the science of their own generation. Both have influenced in a profound and far-reaching manner the science of subsequent generations.

The fact that they have these common characteristics must not lead us too hastily to the conclusion that there is a detailed parallelism in their lives. Neither can the linking of the two names on the

same anniversary program be accepted as evidence that they are equal in intrinsic greatness or in their influence on science. It merely invites us to take stock of the work that the two men did and the movements that they set under way, with a view to deciding—if as individuals we choose to do so—which has contributed the most to the century which has passed and which has most to offer in inspiration and guidance for the future.

To this task we must turn in an effort to do the fullest justice to both men, but with that scientific candor which should characterize our attack on any problem.

II

Here lies our greatest difficulty. Scientific men are after all very human creatures. They fish in the same pools, worship at the same shrines, and sometimes have that almost pardonable human weakness of projecting haloes about a selected few of the human figures in the development of their science which are sufficiently distant in time and obscure in actual personality. In science we have no formally canonized saints. Nevertheless, we must not forget that we are to-day comparing two quite different entities—we are contrasting Sir Francis with Saint Gregor.

Here we reach our first point of contrast between Mendel and Galton. Mendel must be viewed through the halo which unconsciously but none the less definitely has been projected about him since the simultaneous rediscovery of the principles announced in his one noteworthy contribution raised him from almost total obscurity to fame. Galton's life is an open record.[1]

Mendel's life has been so often and so minutely portrayed in all ascertainable details that it will be impossible to add anything to what is already familiar. Galton's life was so rich and varied that it will be impossible to do more in the few minutes granted to me than to give a few illustrations of his achievements.[2] I shall attempt merely to balance the two personalities against each other, leaving the decision as to the relative importance of their work

[1] So detailed is the available information that a well-known psychologist has attempted to determine "the intelligence quotient of Francis Galton in childhood." See L. M. Terman, *Amer. Jour. Psychol.*, 28, 209-215, 1917.

[2] The present paper is in no sense an attempt at an abridged biography of Galton. I hope that it may stimulate the reader to read Galton's charming "Memories of My Life," which will be as fascinating to the general reader as to the professional biologist, and Karl Pearson's masterly biography, "Life, Letters and Labors of Francis Galton," which must remain for all time not merely the source of authoritative information concerning Francis Galton, but one of the most notable biographies of this century. It is much to be hoped that the near future will see the completion of this comprehensive work, long delayed by war activities.

and the relative significance of their influence on science to the jury here assembled.

III

A perusal of the mere facts of the lives of Galton and Mendel at once raises, but unfortunately fails satisfactorily to answer one of the questions in which Galton had a sustained interest—that of the relative importance of nature and nurture in determining the characteristics of the individual. Galton came of stock of long proved intellectual power. Mendel's more distant ancestry is hidden in obscurity, and there is no evidence of great intellectual ability in the parental generation. Galton's family could and did provide for him as a boy the best that the times afforded in intellectual discipline and inspiration. The beginnings of an education were for Mendel a rare opportunity and bought at a sacrifice to his family. Galton saw the world broadly, through his own eyes and through those of his friends—eminent in exploration, in science and in public life. Mendel's outlook was always limited by the walls of a narrow geographical cloister.

Yet both men have made great contributions to scientific advancement.

If for a moment I may be permitted to show partiality, and to venture at interpretation as well as presentation of facts, I must confess that it seems to me that as an individual Mendel deserves the greater credit for his achievement. Nurture fed all that was best in Galton's rich natural inheritance. Intellectual nurture was not lavished on Mendel. There may have been at one time real danger that Galton's abilities would be sterilized by luxury. There was grave danger that Mendel's abilities would never be of significance in scientific progress because of lack of opportunity.

Fortunately neither of these misfortunes was realized. In 1847 Mendel was ordained a priest, and in 1849 he was sent at the expense of his cloister to the University of Vienna, where he remained until 1853. It was in the midst of this period, in 1849, that Galton suddenly ended "the fallow years" of his life, ceased sowing his wild oats, and turned again to the scientific studies which had fascinated him before his interests had been submerged in a life which for a time had been devoted too largely to reckless pleasure.

IV

We should not proceed further with our comparison without noting that neither of the men whose birth is to-day celebrated by the American Society of Naturalists was an avowed naturalist in the more classical sense of the term nor a professional biologist in its modern interpretation.

I have searched in vain through Galton's accounts of his travels for evidence of a compelling interest in the peculiarities of the plants and animals of the extensive region which he opened up to geographical knowledge by his early explorations. He seems never to have described a species, or even to have collected them. His name is represented in the literature of taxonomy only by the South African genus *Galtonia*, of the French botanist Decaisne. It is perhaps significant that as an explorer Galton's natural history observation dealt not with the fascinating superficial forms of the organisms which he encountered, but with animal and human behavior.

Mendel may have been more under the influence of the biology of his own time. He wrote notes on *Scoplia* and *Bruchus* as a student. He was apparently influenced to some extent by the work of other experimental breeders. His materials were at least varied. While his chief work was with plants he devoted much energy to an attempted study of inheritance in bees.

In so far as they considered living organisms the attitude of both Galton and Mendel was more like that of the modern biologist than that of the earlier naturalist.

It is not difficult to establish this, to the complete satisfaction of geneticists, for the Abbot Mendel's greatest contribution, that on inheritance in peas, still stands as a model for much of the modern work in genetics.

Because of the greater number and the wider scope of his publications it is not so easy to pass upon the work of Galton. I fear that many of the members of this audience are unaware of the wealth of Galton's contributions. It will facilitate our subsequent discussion to detail a few of his activities. For the moment I shall limit attention to those which have an immediate bearing on modern biology. His studies of finger prints laid the scientific foundation for a widely applied means of penal and military identification. His early attempts to influence the character of the offspring by the transfusion of blood were essentially modern in their experimental viewpoint. Anthropometric laboratories owe their origin to his interest in human faculties and still profit by his early-devised methods. His studies of the inheritance of physical and mental characters, given to the world over a half a century ago, still furnish, when coupled with an attempt at Mendelian analysis, the model for much of what passes for research in eugenics. Finally, his contributions to the application of mathematical methods in the biological field have had so great an influence that they must be reserved for separate discussion in a later section.

V

The suggestion will inevitably be offered that the work of Galton and Mendel was of the nature of modern biology rather than of the older natural history of their day because of the fact that their pioneer work determined to an appreciable degree the lines along which modern biology has evolved.

Herein lies one of the most interesting points of comparison between Mendel and Galton, and one concerning which there will probably be little differences of opinion among the majority of this audience.

To those who view the vast output of genetic investigation—evident alike in our journals and in our scientific programs—there may seem no reasonable doubt that Mendel has had a more profound and far-reaching influence on biology than Galton.

I have no desire whatsoever to quarrel with the far superior numbers who hold this opinion. I would, however, like to have the facts candidly and judiciously examined.

No one can deny that Mendel's influence on biology—assuming for the sake of argument that the development of modern genetics is to be attributed largely to the influence of Mendel—is conspicuous because of the fact that it has resulted in a narrow and highly unified field of work. Galton's influence was much more varied.

Let us for the moment disregard the broader aspects of biology and of science and limit our attention to the field of genetics, as it has been influenced by the work of Mendel.

Has Mendel's influence transcended or even equalled that of Galton?

Mendel had a relatively small and certainly only an ephemeral personal influence upon the biology or biologists of his time. His published work was, practically speaking, without influence on biology for three decades. Notwithstanding this fact a number of biologists were at the time of the "Rediscovery" more or less actively engaged in experimental breeding. Three have been recognized as co-rediscoverers, and others have been known to modestly lay claim to having been near the great honor. This widespread interest and activity in the experimental attack on the problems of what we to-day call genetics can not have been a matter of accident.

I venture to suggest that there were three groups of influences which determined these activities; the lingering interest in the studies of the earlier experimental breeders; the rapidly increasing economic importance of plant and animal breeding, and the personal influence of Francis Galton and of his writings.

Let us leave for the historian the decision as to which of these was the most important factor. It is sufficient for our purpose to

recall that while Mendel's paper lay unheeded Galton's pen was influencing public opinion and scientific thought. His volumes did not stand with uncut pages. They were read and annotated. "Hereditary Genius," while foreshadowed by essays[3] appearing in the same year that Mendel's paper was read, was first published in 1869. An American edition was issued in 1870, and again in 1891. A revised English edition was prepared in 1892.[4]

While "English Men of Science" might be assumed to be primarily of national rather than of international interest, the English edition of 1874 was followed by an American reprint of 1875. His more technical volumes were issued in only one edition, but were widely read. They appealed to the more intelligent general reader, and they influenced the thought of the specialist. The volumes of Bateson and de Vries issued prior to the "Rediscovery" bear witness to their influence. Whitman's personal copies of "Natural Inheritance" bear evidence of intensive study. A perusal of the "Foundations of Zoology," penned in part over a quarter of a century ago, shows clearly how great was the influence of Galton on the thoughts of such a leader as W. K. Brooks.[5]

Nor was Galton's influence limited to that of his own writings. Weldon's earlier papers had appeared, and biology was beginning to feel the influence of Karl Pearson's pen. These were, to be sure, largely independent, but they showed, and their authors gladly admitted, Francis Galton's friendly influence.

Whether Galton and Pearson were wrong in regard to theories of heredity, or in their method of attack upon the problem of inheritance, does not concern us here. The thing which is of real importance for our present consideration is that fact that in 1901 the scientific world was ready to replace speculation on inheritance by inductive research. Some force or forces led to the profound changes in biological thought between 1866 and 1901. Certainly these changes, which may have been greater than those which have taken place since Mendel's work received recognition, were not due to Mendel. I venture to express the conviction that among these forces one of the most powerful was the direct and indirect influence of Francis Galton.

It was no fault of Mendel that the circumstances of the "Rediscovery" and subsequent events have tended to obscure the in-

[3] Galton, F.: "Hereditary talent and character," *Macmillan's Mag.*, 12, 157-166, 318-327, 1865.

[4] An important prefatory chapter in this edition gives Galton's impressions of the activities of the twenty-three years which had elapsed since the appearance of the original edition.

[5] While Brooks did not always agree with Galton, he wrote: "My own debt to Galton is great, and it is acknowledged with gratitude."

fluence of Galton. The practically simultaneous announcements of Correns, Tschermak and de Vries, coupled with the discovery that Mendel had preceded them by three decades, made a spectacular setting for the new field of experimentation—a staging which was further illuminated by Bateson's controversial writings.[6]

Thus the circumstances of the "Rediscovery" were such as to place Mendel at once in the most conspicuous place on the biological stage.

From 1869 when "Hereditary Genius" appeared until 1889 when "Natural Inheritance" was given to the world and to 1901, which marked the founding of a special journal for the statistical investigation of biological problems, Francis Galton had prepared the professional and the lay mind for a dominant interest in heredity. In 1901 the seed of the "Rediscovery" fell in fertile and well-tilled soil, and Mendel reaped where Galton and his coworkers had cleared and tilled.

VI

We have already considered in a preliminary way the influence of Galton and Mendel on genetics. Mendel's direct influence has extended little beyond this one phase of biology. Galton's work and influence in biology and in science were much broader. Few biologists realize their scope, or their importance.

VII

First of all, Galton early won recognition as an explorer. While Mendel was completing his studies at Vienna, Galton was traversing the land of the Namaquas, the Damaras and the Ovampo. His trail led him over a thousand miles into tropical Southwest Africa. The regions which he traversed were mapped on the same plate which gave to the world the geographical results of some of Livingston's explorations.

It is our loss that we can not stay to review some of the fascinating chapters of his "Tropical South Africa,"[7] a volume in which

[6] Those who know the history of biology in the quarter of a century now coming toward an end must realize that the simultaneous rediscovery of Mendel's principles by three different investigators lead to an emphasis upon the priority of Mendel's work which would not have been laid if only one more recent worker had observed the agreement between the frequencies of individuals of different classes in the segregated generation and those given by permutation formulæ.

[7] Galton, F.: "The Narrative of an Explorer in Tropical South Africa," London, Murray, 1853. The first edition was quickly exhausted, but a reprint, with a few changes, was issued in the *Minerva Library of Famous Books* in 1889. A valuable appendix, prepared by Mr. Galton, gives additional information of interest obtained during the thirty-six years which had elapsed since the preparation of the original volume.

the hardships and adventures of the twenty months of exploration are subordinated to serious observations on the peoples and their customs.

Impaired health could not prevent Francis Galton from contributing to the advancement of geographical science, though it did lead him to decline the opportunities for further African exploration. His editorship on "Vacation Tourists" was of but short duration, but the contents of the three volumes justified the editor's suggestion that "scientific tours offer an endless variety of results." His "Art of Travel,"[8] while in a sense a compilation, in part from the literature and in part from the personal experiences of the great explorers of his time (with whom he was in intimate association), is truly remarkable not merely in clearness, conciseness and comprehensiveness, but in conception.

In evaluating the significance of his conception we must not forget that the volume was prepared at a time when the Royal Geographical Society was but twenty-five years old, when only the margins of great continental areas were even roughly mapped, and when vast wildernesses, including much of our own fertile domain, were open for colonization. Writing at this time—which was long before the acceptance of the idea that technical training might constitute a part of education—Galton wrote in his preface to the second edition:

> I am convinced that the Art of Travel, or of campaigning, admits of being taught, . . . It therefore seems to me, though I may perhaps be considered an enthusiast by many, that every intelligent youth who seeks a commission in the army, or to become an emigrant, or a missionary, should find his time and energy well spent in learning to use the axe, saw and chisel, the soil needle, the cobbler's awl, the blacksmith's hammer and the tinsmith's soldering iron together with the greater part of the bush manufactures and makeshifts of which this volume treats.

That the volume fulfilled the necessary requisite of usefulness is perhaps evidenced by the several printings which have been issued.

VIII

Time will not permit a detailed discussion of Francis Galton's work in the physical sciences. His studies can not be judged by the standards of physics and chemistry, for they have had only an insignificant influence upon the development of these sciences. Neither can his contributions be dismissed as the pastimes of an

[8] Galton, F.: "The Art of Travel; or Shifts and Contrivances Available in Wild Countries," London, Murray, 1855. Various subsequent editions.

ingenious amateur. Practically without exception they indicate inventive efforts towards the practical application of physical principles in other sciences.[9]

Among such studies we may read by title only his work on a hand heliostat for signaling on sea or land, his principle for the protection of riflemen, stereoscopic maps, spectacles for divers, the conversion of wind charts into passage charts for vessels of known sailing capacities, his work on a drill pantograph, instruments for determining the upper limit of audible sound, composite portraits, photographic measurement of animals, and analytical photography.

Galton's work in the physical sciences was not characterized merely by inventive skill. He was keenly alive to the necessity for the standardization of instruments at a time when little attention was given to this essential outside the physical laboratory, and he gave personal attention to the rating of watches, and the standardization of sextants and clinical thermometers, as well as to administrative work during his long connection with Kew Observatory.

IX

The widespread interest in the weather and the chaotic state of meteorological work at the time of Galton's return from his African explorations were almost inevitably a stimulus to his interest. He saw at once the importance of synchronizing and standardizing observations made at stations as systematically distributed as possible over wide areas. He foresaw instinctively the necessity for a better organization of the data then available if progress was to be made in meteorological investigation.

It will be practically impossible for biologists, accustomed to turning to a wealth of meteorological observations tabulated and mapped in detail for their use in studies of geographical distribution, to realize the conditions which prevailed at this time. Partly for its intrinsic interest and partly for its illustration of Francis Galton's firm grasp of scientific method and the importance of scientific organization and cooperation, I beg permission to quote

[9] His earliest paper, printed after his departure for his early African explorations, in 1849, was on the possibilities of a printing telegraph instrument. The apparatus designed seems never to have been carried beyond the preliminary stages. The point of greatest interest lies in the fact that seventy years ago Galton foresaw clearly the possibilities of house-to-house communication which might result from a system of centrals if the telegraph instrument could be adapted to the use of individuals instead of limited to highly trained operators.

the following from the introduction to his "Meteorographica"[10] of 1863:

A scientific study of the weather on a worthy scale seems to me an impossibility at the present time from want of accessible data. We need meteorographic representations of large areas, as facts to repose upon, as urgently as experimental data are required by students of physical philosophy.

Meteorologists are strangely behindhand in the practise of combining the materials they already possess. There are more than 300 skilled observers, using excellent instruments, scattered over Britain and the continent, who transmit observations taken twice daily to meteorological societies or government institutes. Besides these, are the same number of lighthouse keepers.... Lastly, many observers publish independently. Yet throughout this mass of labor that practise of general combination is absent, which is required to utilize it as it deserves. No means exists of obtaining access to any considerable portion of these observations, without great cost, delay and uncertainty, much less can they be obtained in a "reduced" and never in a meteorographic form. The labor of a meteorologist who studies the changes of the weather is enormous before he can even get his materials into hand and arrive at the starting point of his investigations. In the ordinary course he has to apply, with doubtful chance of success, to upwards of ten meteorological institutes in Britain and Europe, for the favor of access to the original documents received by them, and to fully thirty individuals besides. He has next to procure copies, then to reduce the barometer and thermometer readings to a common measure, and, finally, to project them on a map.

I feel that all this dry, laborious and costly work, which has to be undergone independently by every real student before he can venture a step into the scientific part of his work, is precisely that which should be undertaken by institutes established for the advance of meteorology.

After discussing some of the essential features of meteorological maps he says:

A sustained series of publications of this kind, extending over two or three years, would give an extraordinary impetus to the scientific study of meteorology. They would supply the necessary materials in a manageable form, for arriving at a general knowledge of the distribution of the various elements of the weather; they would afford means of testing the extant theory of "forecasts" with a rigor impossible at the present time, and they would necessarily improve it.

Galton foresaw the desirability and the possibility of international cooperation, for he continued:

If extensive tables of reduced observations were issued in England we might look for the cooperation of meteorological institutes on the continent (who already publish voluminously) in following our example.

After discussing some of the practical problems of international cooperation, he concludes:

Entertaining the views which I have expressed on the necessity of meteorological charts and maps, and feeling confident that no representation of

[10] Galton, F.: "Meteorographica, or Methods of mapping the weather"; illustrated by upwards of 600 printed and lithographed diagrams referring to the weather of a large part of Europe. London and Cambridge, 1863.

what *might* be done would influence meteorologists to execute what I have described, as strongly as a practical proof that it *could* be done. I determined to make a trial by myself, and to chart the entire area of Europe, as far as meteorological stations extend, during one entire month . . .

In evaluating the conception we must not forget the time at which it was written: It was two years before the two essays which furnished the first published intimation of "Hereditary Genius" and before Mendel's paper was read. It was at a time when for our own meteorological records we were depending upon the journals of our exploring expeditions and surveys, upon the observations of our medical officers at the army posts, and upon such systematic records as the Smithsonian Institution could assemble. Thus Galton's personally printed weather maps preceded by over twenty years our first tridaily meteorological maps.[11]

But we must hasten on; it is not our purpose to detail before a biological audience his various activities in the development of modern meteorology, during the more than thirty-four years of his activities at Kew Observatory and in the Meteorological Office.

In writing to Mr. Galton upon his resignation from the Meteorological Office Sir Richard Strachey said:

> It is no exaggeration to say that almost every room in the office and all its records give unmistakable evidence of the active share you have always taken in the direction of the operations of the office. The council feel that the same high order of intelligence and inventive faculty has characterized your work in meteorology that has been so conspicuous in many other directions, and has long become known and appreciated in all centers of intellectual activity.

I am not pretending that Francis Galton was great as a meteorologist. His work came too late in the development of the science to appear alongside that of Galileo where it might be expected in the indices of modern text-books. His personal work came too early to be connected with much of the modern development which has resulted from the widespread establishment of stations which he foresaw as an essential to the practical use of meteorological observations in the protection of shipping. It is not unreasonable to suppose that Galton's work will have greater influence upon the future development of meteorology than it has in the past, for the methods of correlation, regression and partial correlation are now being introduced into the treatment of meteorological data.

Looking back on these phases of Mr. Galton's public service,

[11] U. S. War Department, Office of the Chief Signal Officer: *Tridaily Meteorological Record*, issued from the Office of the Chief Signal Officer. The charts begin with January, 1878, but were not published until 1884.

we as naturalists can not but regret that it absorbed energy which might have been devoted to studies of inheritance, and to the problems of eugenics.

But let us not forget that the scientific man is a citizen as well as an investigator. Mr. Galton's gift of time and thought to administrative duties curtailed his list of books and papers.

Have they curtailed his influence upon the progress of science?

X

I have touched as lightly as possible on those achievements of Galton which had no counterpart in Mendel's work, in part because I have wished to avoid any possible semblance of partiality in the treatment of the two characters and in part because I have desired to reserve the time for a discussion of the broader aspects of Galton's biological work.

In Galton's narrative of his African explorations there is, as I have said, little to indicate a keen interest in the flora or fauna, other than the big game. There are, however, unmistakable evidences of interest in man. To Galton the loss of valuable equipment was not merely a difficulty to be overcome. It was an opportunity to study the behavior of primitive peoples, by pitting one tribe against another in the recovery of his lost property.

In his later life this interest made itself felt in four interrelated fields of work—in anthropology, in psychology, in heredity and in eugenics.

Galton's studies in these four fields are so closely interrelated that in noting his major contributions I shall follow in the main a chronological sequence.

An illustration of the impossibility of separating them is afforded by "Hereditary Genius," his first great biological work, which appeared sixteen years after his "Tropical South Africa," and during the period of his most active interest in meteorology.

"Hereditary Genius," as Mr. Galton tells us, grew out of a purely ethnological inquiry into the peculiarities of diverse races. In 1874 "Hereditary Genius" was supplemented by a volume on "English Men of Science; their Nature and Nurture." In 1883 "Enquiries into Human Faculty" assembled under one cover the observations, experiments and statistical studies of color blindness, capacity for distinguishing shrill sounds, criminality and insanity, gregariousness and slavish instincts, number forms, the sensitiveness of blind and seeing and of savage and civilized individuals and many other interesting topics.

It was during this period that his interest in anthropological measurements and in finger print identification developed. In

1882 he published a plea beginning "When shall we have anthropometric laboratories where a man may from time to time get himself and his children weighed, measured and rightly photographed, and have each of their bodily faculties tested by the best methods known to modern science?" As a result of his interest, an anthropometric laboratory was established. He recognized the possibilities of obtaining materials of value for scientific investigation from measurements carried out in the public schools, and utilized such materials in his own studies.

Coincident with and supplementary to physical measurements were his studies of finger prints.

Galton foresaw the difficulties of the use of the Bertillon system because of the fact that physical measures are correlated. His interest in finger prints as a means of personal identification first became generally known through a lecture delivered before the Royal Institution in 1888. In 1891 he published an extensive memoir in the *Philosophical Transactions*. His books on the subject are "Finger Prints" (1892), "Decipherment of Blurred Finger Prints" (1893) and "Finger Print Directories" (1895).

The consequence for science of these studies is not to be judged by the volume of the data, by the pages published or by the conclusions drawn. Neither will I point to the fact that in our recent world conflagration the possibility of the identification of millions of men depended upon methods for which we are primarily indebted to Galton.

The true importance of his work inheres in more subtle influences. First, these studies contributed, directly and indirectly, to the development of Galton's own grasp of the larger problems which I shall discuss in a moment. Second, they had an immediate influence upon the scientific thought of his own time.

All students of Mendel's life have emphasized the fact that his influence upon his contemporaries was but insignificant.

The case is quite different with Francis Galton. Throughout his life he was active in the affairs of the British Association, the Royal Geographical Society and the Anthropological Institute. During his later years when his own pen was less active his personality was an inspiration to the scientific men with whom he came in contact, and many of them owed their success in no small degree to his kindly interest. They, in their turn, extended his influence. In America, for example, the early development of both experimental psychology and of statistical methods in anthropology is in no small degree due to J. McKeen Cattell's early association with Francis Galton.

It is hardly too much to say that the personal influence of Galton was one of the chief factors in the development of anthro-

pometry from anthropology. His influence upon the development of experimental psychology was probably equally great.

XI

This brings us to what is the next to the most important if not the most important phase of Galton's work and influence.

The facts which any one individual accumulates can generally be discarded without serious loss within a few years after the good which he has done is interred with his bones. While Galton's contribution of facts constitutes a much larger fraction of the literature of biology than does that of Mendel, it has long since been replaced by better or more extensive data and his name rarely appears in the "Literature Cited" of our current journals. Galton's really great contribution was that of method.

Here again we arrive at a certain parallelism between Galton and Mendel. In their influence upon the methodology of science Galton and Mendel had two things in common.

First, they both recognized the value of a direct appeal to experimental or observational facts as contrasted to speculation and authority. They differed in that Mendel appealed to experimentally determined facts, whereas Galton laid greater emphasis upon the statistical analysis of observations and measurements.[12]

Second, both showed biologists the value of replacing the irregularities of observational frequencies by a smooth mathematical formula.[13] A permutation formula—for example, that leading to the 9:3:3:1 ratio—is a mathematical expression fitted to the empirical data just as truly as is the normal curve.[14]

XII

Galton's work in the application of mathematical methods to biological problems extended much farther than the mere description of frequency distributions by mathematical formulæ. He

[12] The difference was in no way due to a lack of sympathy on the part of Galton with experimental methods, but rather to his keen interest in man—an organism which is not available for experimentation in the same way as are peas.

[13] Probably the greatest difficulty of some of those who preceded Mendel and who almost attained his results was that they could not see the forest because of the trees. The data obscured the laws.

[14] An amusing feature of the criticisms of the biometric school by Mendelians is the fact that the critics have failed to realize that the features of both methods of approach which are of the greatest importance in science are the same—namely, the replacement of the confusing irregularities of empirical frequency distributions by the illuminating regularity of mathematical expressions which represent the results with a degree of accuracy which lies within the limits of the probable errors of random sampling.

grasped the important conception that in biology we are chiefly concerned with the degree of interrelationship between the variables with which we have to deal. His conception of correlation and regression, vastly extended and enriched by Pearson of the Biometric School, has given us some of the most powerful tools in biological research.

There is no time at my disposal for a history of biometry, nor is this the occasion for an indication of what biometry has accomplished or what it is capable of accomplishing for biology in the future. It is sufficient for the moment to point out that many thoughtful biologists recognize the fact that the progress of science depends not merely upon the accuracy of measurement and the closeness of control of experimental conditions, but upon the adequacy of the mathematical description and analysis of the observational, assembled toward the solution of a given problem. This was not obvious in Galton's day. He foresaw in part the long and bitter fight which would be necessary to establish mathematical analysis as an essential part of biological investigation, but the campaign fell to the lot of younger men. It is to his glory to have had the vision. It has been the opportunity and the privilege of his followers to wage the battle.

XIII

In many of the comparisons hitherto drawn there is some element of parallelism, however approximate; some semblance of equality, however slight, between the achievements and the influence of Mendel and Galton. When we turn to Galton's greatest potential contribution of human advancement there is no possibility of comparison further than to note the significant fact that it was not the ecclesiastic, Mendel, but it was the biologist, Galton, who had the vision to foresee the possibilities of the improvement of human stocks under present conditions of law and sentiment.

Thus what may become the most fundamental service of science to humanity was not foreseen, or at least not formulated or fostered by one whose vocation was religion but by one whose vocation was science.

Nor was it merely a question of vision. Galton not only foresaw[15] the possibilities of the application of the laws of biological

[15] It is an interesting fact that in his first paper on inheritance (1865) Galton wrote: "The power of man over animal life, in producing whatever variety of form he pleases, is extraordinarily great. It would seem as though the physical structure of future generations was almost as plastic as clay, under control of the breeders' will. It is my desire to show, more pointedly than—so far as I am aware—has been attempted before, that mental qualities are equally under control" (p. 157). On another page of the same essay we

science to the improvement of human stocks, but he had the energy, courage and practical wisdom to make such provision as he could for the realization of his vision. He himself contributed extensively to the research[16] which should precede propaganda[17] and must precede legislation or action. His contribution was not merely the products of his own pen, but the personality drew about him a school whose researches contributed to the advancement of the science in which he was interested as rapidly as the care which such work demands made progress possible. Finally, he provided that the major portion of his private fortune should be devoted to the foundation of the first laboratory for national eugenics, and named as his personal choice for its head one of the ablest and most productive scientific men of our generation.

This is neither the place nor the time to discuss in detail the work of the Galton Laboratory, where the highest ideals of rigorous investigation have been steadfastly maintained, or the status of eugenics as a science. It is enough to say that the interest engendered by the pioneer work of Galton, vigorously forwarded at a later date by the work of Karl Pearson and by the establishment of the Galton Laboratory, has been the direct cause for the establishment of a series of similar institutions throughout the world.

Finally, let us say to the credit of Galton himself that while his earlier writings were too far in advance of their time to receive serious consideration he did not succumb to discouragement. In the later years of his life he might have been the hero of a mob of enthusiasts, but he was willing to be patient and to wait for research to lay the needful foundations.

XIV

Which was the greater man? Which has influenced most profoundly the development of science as we know it to-day? Which

read: "I hence conclude that the improvement of the breed of mankind is no insuperable difficulty. If everybody were to agree upon the race of man being a matter of the very utmost importance, and if the theory of the hereditary transmission of qualities in men were as thoroughly understood as it is in the case of our domestic animals, I see no absurdity in supposing that, in some way or other, the improvement would be carried into effect."

[16] Galton's chief volumes have been cited above. It would lead us too far from our purpose to attempt to cite or review the minor papers which show the development of his interest in and appreciation of the problems of eugenics.

[17] Galton was aware of the dangers of propaganda. Thirteen years ago, when some of his essays were assembled in a little volume, "Essays on Eugenics," by the Eugenics Education Society, he wrote in the preface: "It is above all things needful for the progress of eugenics that its advocates should move discreetly and claim no more efficacy on its behalf than the future will confirm; otherwise a reaction will be invited."

has set in motion the more important forces for the future development of science?

Personally, I have no shadow of doubt as to the correct answers, but I shall not obtrude my own opinion upon you.

After all, the questions do not demand formal answers. Let us instead content ourselves by repeating a sentence from our introductory remarks: "Both men worked in advance of the science of their own generation. Both have influenced in a profound and far-reaching manner the science of subsequent generations."

It is fitting that the American Society of Naturalists should do them honor.

A PERMANENT MEMORIAL TO GALTON AND MENDEL

By Professor GEORGE H. SHULL
PRINCETON UNIVERSITY, PRINCETON, N. J.

WHEN the Mendelian principles of heredity were simultaneously rediscovered and promulgated in 1900 by de Vries, Correns and Tschermak, the systematic study of variation and heredity had been already for several years proceeding with vigor along a wholly different line, known as biometry. As increasing numbers of zealous workers in the Mendelian field made it increasingly evident that the principles discovered by Mendel had widespread if not general validity, the inevitable conflict between the Galton-Pearson methods of attack on the problems of genetics and the Mendelian methods engendered much bitterness and even as late as 1909 an "Ardent Mendelian" depicted the devotees of biometry and those of Mendelism as two armies pitted against each other in mortal combat with ultimate victory inevitable for the Mendelians. We can not admit that this picture ever accurately represented the attitude of biometricians or of Mendelians, generally, though in individual cases it may perhaps have been justified.

What a change hath the past decade wrought! With keen satisfaction we bracket together to-day the names of Galton and Mendel with the assurance that in so doing we give offense to no one! *Mendelism has indeed won the victory, but so also has biometry!* And instead of mortal combat, there has come fraternization, mutual understanding, cooperation, even amalgamation, so that to-day the names of Galton and Mendel stand as twin pillars in the basement room of the house of modern biology, which has been christened "Genetics."

To geneticists it is well known that the fundamental concept in both biometry and Mendelian heredity is one and the same concept; to other biologists this may not have been fully appreciated as yet. Both biometry and Mendelian heredity involve the amassing and analysis of statistics, and the analysis in both rests on the assumption of independent assorting of pairs of alternatives. The basic principle of both is the principle known as the "law of chance," a principle which might be less easily misconstrued if it were always spoken of as the "law of probability," since the word "chance" connotes to many the antithesis of causation whereas in reality it assumes that even events which are individually unpredictable are due to the interplay of definite causes. The simplest phase of this principle may be represented by the repeated tossing of a coin which may fall either "head" or "tail." Cases of increasing complexity may be as simply illustrated by the repeated tossing of two coins at a time, then 3 coins at a time, 4 coins at a time, and so on, to any degree of complexity desired. Mathematically, the results are symbolized by the expression $(a+b)^n$ in which a and b may each be assumed to have the value unity. Mendelian phenomena, as practically handled, usually involve only the lower values of n in this formula (the number of allelomorphic pairs) and also generally require that a and b be of such character or magnitude that they may be easily distinguished from each other. Biometry, on the other hand, works ideally with cases in which n is large, and the alternatives represented by a and b are not easily or not at all differentiable. But there is no *natural* line of distinction between cases in which n is small and those in which it is large, nor between the cases in which a and b are easily distinguished and those in which they are not easily or not at all capable of distinction. The combination of biometry and Mendelism in modern genetics has resulted therefore quite naturally from extended experience in both fields with mutual invasion and overlapping.

The significance for modern biology of the introduction of statistical methods has been well discussed by the last preceding speaker and I will attempt to add nothing to this; but rather take occasion to point out a certain interesting parallel and antithesis in the lives of the two men the centenaries of whose births we celebrate to-day.

Not only were Galton and Mendel born in the same year (1822), but they also published their first scientific contributions at nearly the same time, and at a time that must be considered in these days relatively late in life, namely the age of 43. Mendel's classic paper was read in 1865 and printed in 1866, while Galton's first papers on the statistical study of heredity were published in *Mac-*

Millan's Magazine in June and August, 1865, but the existence of these would be now as completely buried in oblivion as was Mendel's paper before the "rediscovery" were it not that they were mentioned by Galton in a book that has lived, entitled "Hereditary Genius," published in 1869.

Aside from the coincidence of these two cardinal dates (their births and the dates of their first publications) in the lives of Galton and Mendel, their biographies are much more striking in their strong contrasts than in their parallelism; for Mendel was the son of a peasant, while Galton was born to a family already well endowed with the results of several generations of energetic commercial life and thrift. Mendel continued but a short time in the execution of genetical experiments after the publication of his greatest paper; Galton continued his studies throughout a long life and contributed books and magazine articles almost to the day of his death, and in his will he endowed a laboratory to continue in perpetuity the type of research which he had inaugurated. Mendel died in 1884 at the age of 62, wholly unknown to the scientific world; Galton, on the other hand, died in 1911 at the ripe age of almost 89 years, and was well laden with the various honors which admiring scientific and governmental organizations could bestow.

There is something fine in the spontaneity which has been shown by scientific organizations, both national and international, in taking cognizance of this centennial year. All the genetical journals and a number of other biological journals have celebrated in one form or another, and several special commemorative programs have been arranged. The most notable of these was the international gathering in Brünn, Czechoslovakia, in September last.

It is to be noted, however, that these various activities represent quite ephemeral manifestations of the great respect and admiration we all feel for Galton and Mendel. The question has been raised whether these sentiments ought not to be expressed in some more enduring form, and the answer has seemed to be inevitably in the affirmative; but what should be the nature of such a memorial?

Tablets of bronze, and busts or statuary of bronze or marble, appropriate locally to mark the places of birth or of labor, are hardly appropriate for us who have in our midst no *places* to thus commemorate in association with the names of these two great spirits. There is left, however, an even better association than that between name and place, to which to attach a memorial, namely, the association between the men and the *work* to which they gave their devotion and which laid the foundations of a new

branch of biological science. It is suggested, therefore, that the best type of memorial is one which will, in perpetuity, promote the sort of scientific activities to which they were devoted.

It is a fact now well known in scientific circles that the research journals are in difficulty, and unable to meet the current needs of research men for the recording and promulgation of their discoveries. It is recognized that this condition seriously menaces rapid progress in scientific discovery. *Genetics*, particularly, has been unable to meet the needs of geneticists who work with color characters, nor has it been able without special gifts either from the authors themselves, or from other outside sources, to publish the expensive tabular matter required in a statistical science. It will be recalled that in Mendel's original study, four of the eight alternative characters which he considered in the garden pea were color characters, and modern geneticists have likewise found pigmentation characters forming a considerable portion of their most instructive research material. Gross verbal descriptions of such pigmentation characters serve fairly well the crude preliminary studies, but as the analyses become sharper and more detailed such verbal descriptions are certain to lose much of their value. It is proposed therefore as a permanent memorial to the founders of the science of genetics to ask for popular subscriptions to a "Galton and Mendel Memorial Fund" the principal of which shall be kept invested in perpetuity, the income from which shall be devoted to the publication of such colored plates and other expensive types of engraving as may be necessary in the illustration of research papers in the journal *Genetics*, and also to defraying in part the cost of publishing necessary tables of statistics.

There is a special reason why this is a particularly appropriate type of memorial for Gregor Mendel. Several suggested explanations have been offered to account for the long submergence of Mendel's magnificent contribution, and all such explanations doubtless have a certain degree of validity, but the explanation which seems most generally acceptable is that of defective publication. It is true that the *Transactions* of the Natural History Society of Brünn were being exchanged with no less than 120 other societies and institutions of similar scope or related purpose; but it is hardly to be doubted that had his paper been published in one of the standard biological periodicals of the day it would not have remained for 35 years unknown to others capable of appreciating its bearing.

The significance of the type of memorial here proposed will become clearer when we consider what becomes of the papers which can not be published in any of our standard journals because of the need for expensive types of illustration, or for unusual quanti-

ties of tabular matter. Some American papers requiring colored plates have been sent to England or Germany and published in foreign journals; but it is obvious that these sources of publication facilities are strictly limited. Probably the greatest number are published without the illustrations and tabulations required for the full realization of their value. A number of papers with colored plates or other expensive engravings have been published as University Bulletins, the method of issue of which is similar to that of Mendel's original paper, and for this reason they may be expected to be less readily accessible as the years go by than if published in a standard journal like *Genetics*. Have we any assurance that we are not burying in this way to-day important contributions which may or may not be rediscovered, several decades hence?

The amount of money needed to meet present requirements of illustration and tabular matter in *Genetics* has been estimated at $50,000 and this sum is therefore set as the goal to be aimed at.

Is it not clear that since the contributions of Galton and Mendel and the science which they founded touch vitally every branch of biology, giving new viewpoints and new outlook, it is proper to invite all biologists and all those interested in the applications of biology to cooperate in the establishment of this Memorial Fund?

A considerable number of contributions ranging from ten to one hundred dollars each have been received already from biologists, as well as from others who, though not biologists themselves, have an interest in the progress of biological discovery. Since it will be impossible to appeal by individual letters to all of the thousands of biologists in the country, each reader who has not already sent a donation is asked to consider this a direct personal appeal for a contribution to the Memorial Fund. The fund will have its proper character as a memorial only if (or in proportion as) the list of donors contains the names of all who understand and appreciate the epoch-making work of Galton and Mendel.

A permanent list of donors will be kept in which the following grades will be maintained: Persons contributing $5,000 or more will be designated Founders; those giving $1,000 or more, Patrons; $100 or more, Supporters; $10 or more, Contributors; and those who give less than $10 will be listed as Associates.

Checks or other valuable securities, such as stocks or bonds, should be made payable to the Galton and Mendel Memorial Fund and sent to the Secretary of the Editorial Board of *Genetics*, George H. Shull, 60 Jefferson Road, Princeton, New Jersey, who will render prompt acknowledgment.

The permanence of the memorial will be insured by placing the

fund in the hands of a conservative Trust Company, and only the income will be used as needed for the purposes stated. Formal acknowledgment will be made whenever any part of this income is used, and provision will be made also that in the event that it can be no longer appropriately used in the manner here designated the fund will pass to the trusteeship of the American Association for the Advancement of Science for reallocation in such manner as will in its judgment best serve as a memorial to Galton and Mendel.

It is believed that this very practical type of memorial will meet the approval of all those who realize that the best memorial to a scientist is one which serves to promote the work in which he was interested. It is also believed that biologists will generally esteem it a privilege to enroll themselves among the admirers of these two great prophets of modern biology—Francis Galton and Gregor Mendel.

PASTEUR, THE MAN[1]

(DEC. 27, 1822—SEPT. 28, 1895)

By Dr. ERWIN F. SMITH

U. S. DEPARTMENT OF AGRICULTURE, WASHINGTON, D. C.

IN every country and every age the really great men are few. By *great* I mean those who in intellectual and moral grandeur tower like mountain peaks above their fellows. The only exceptions are Athens in the time of Pericles and Florence in the Renaissance, when great men abounded. France in the nineteenth century was no exception. If we exclude Napoleon, who really belongs to the eighteenth century, we have Hugo and Pasteur as the great outstanding figures. If we judge these two men by the influence they exerted on their own generation, Hugo would be the greater; but in a long perspective of years Pasteur looms immeasurably larger because of his influence on practical life in many directions. Hugo will always be a great lyric, epic and dramatic poet, the founder of a new school, the poet of democracy and of the future man, the one who came the nearest of any to representing the whole of his time; but theories of versification and theories of all types of artistic and of literary creation vary from age to age and in the long run Hugo, like Balzac, will be only one of many great men who have influenced the trend of intellectual life in a generation. On the other hand, Pasteur's researches on polarization of light, the structure of the molecule and the constitution of matter, on fermentations and the best methods of conserving foods and drinks, his investigation of spontaneous generation, of aërobism and anaërobism, of the cause of human and animal diseases, and his discovery of various methods of controlling such diseases applicable to medicine and to surgery, will bulk larger and larger as time goes on. It is for this reason that men at this time are assembling in all parts of the world to do him honor. It was his immense and unique contribution to human knowledge in a dozen different fields that made Pasteur the companion and equal of kings and emperors and of all the intellectually great men of his time. So much has been written and said and will be written and said soon about the various types of

[1] Read under the auspices of the Graduate Club at Rutgers College, New Brunswick, N. J., December 20, 1922.

new research which Pasteur inaugurated and all the wonderful contributions he made to the advancement of scientific knowledge in new fields that I may be excused if I turn from these things and speak only or chiefly of the kind of man he was, and this especially for the encouragement of students.

Ancestry

Pasteur was a plain man of the common people. He came of a prolific race of hardy hill-people, peasants, small farmers, artisans. There were no scholars among them. His father, grandfather and great-grandfather were tanners.

As a young man his father was a conscript and fought under Napoleon in the war in Spain in 1812-13. He was advanced from the ranks to the post of sergeant major and was decorated for bravery. To him as to many another old legionary the emperor was always a kind of demigod. The father was by nature incommunicative, secret, laborious, devout, of a slow reflective spirit, given to melancholy and the inner life, but full of strong feeling, and at times of fierce action—witness the sabre episode. Many of these traits he transmitted to his son, especially his love of labor and his meditative taciturnity alternating with periods of exuberant and dominating speech. The father could read and write and liked to draw and paint. He was an unselfish man, very considerate of others and tender-hearted to animals. He would not hunt, and the sight of a wounded lark made him sick. When Pasteur proposed to give lessons in the Barbet school out of gratitude for favors received, the father writes, "You owe it not for this reason alone but for the sake of others, since thereby the principal may be induced to extend a helping hand to other studious young men without which their future might be compromised." Again he writes: "Start a subscription in your school for those poor Polish exiles who have done so much for us. It would be a good deed." Once more he sends good advice, greatly fearing the corrupting influences of the Latin Quarter of Paris, and his son replies characteristically: "When one has red blood in his veins he remains simple of heart and upright wherever he is. He who has no will power is the one who yields." The father was often worried lest his son's inordinate desire for knowledge, or, as he puts it, "immoderation dans le travail," should overtax his physical powers and once he writes very wisely: "Les moins pressés sont quelquefois les plus sages."

Again, when the son sends home money from Paris, saved from his meager income, the father writes: "For my part I had a thousand times rather you had kept the money for your own needs," but adds, "Very few parents have the good fortune to have

to say such things to their sons in Paris. And so I am pleased with you more than I can tell you."

In 1843 the father writes: "Tell Chappuis [Pasteur's bosom friend interested in philosophy] that I have bottled some of the 1834 bought expressly to drink to the honor of the Normal school in the first vacation. There is more sparkle in these hundred litres than in all the books of philosophy in the world. But as to mathematical formulas [Pasteur's study] I believe there is not any in them. Tell him we shall be glad to drink the first bottle with him. Be always good friends."

Pasteur's mother was the daughter of a gardener accustomed to work in the fields, simple, affectionate, lovable, industrious, imaginative and enthusiastic, or lively, as we might say. Pasteur was the third of five children. They were poor and the days were long and the mother wrote seldom, but always with a wealth of affection for the beloved son who, far away, was winning friends among eminent men and making a great name for himself as a hard-working and brilliant student. In 1848, toward the end of her life, the mother writes: "My dear child: I trust you will have a successful year. Take good care of your health. . . . Have I not a right to be worried, far from you and unable to give you a mother's care? Sometimes I console myself for your absence by reflecting what great good fortune has been mine to have a child who has gained a position which makes him so happy. . . . Whatever happens to you do not grieve. All is a chimèra in this life."

Boyhood and Young Manhood

As a boy Pasteur played about the tannery and the small stream that flowed past it, and in the great wave of homesickness that overtook him when he first went to the Barbet school in Paris in 1838 all his thoughts were on the odor of the tan bark. "I believe I should be well again," he said, "if I could only smell the odor of the tannery." So his good father went to Paris and brought him back to the village where he spent some time before again venturing on the great whirlpool of the outside world and then he went not to Paris but to Besançon, so as to be nearer his home. In these early years he studied and worked and sketched, making many pastels of people he liked, some of which are very good. At 13 his most striking aptitude was a love of drawing.

It has been said that Pasteur was dull in his youth. The same was said of Sir Walter Scott, and in both cases the dulness was in the master and not in the pupil.

Pasteur was not a brilliant student and only his friend Chappuis guessed his future greatness but he was a very persistent student

and he loved books. As a mere boy we read of his buying dictionaries and grammars and proudly writing his name in them, and, a little later, of his reading Silvio Pellico's "My Prisons," X.-B. Saintine's "Picciola," a book that was still famous in my boyhood, Lamartine's "Méditations" and Droz's "Essay on the Art of Being Happy."

Lamartine as a political leader also stirred his enthusiasm and influenced him greatly. At this time in Paris he joined the Garde Nationale, gave to the country all his savings—150 francs—and then, as ever, was strongly patriotic.

Pasteur was graduated bachelor of letters at Besançon in 1840 with the following comments on his standing: "Good in Greek in Plutarch, in Latin in Virgil, good in rhetoric, mediocre in history and geography, good in philosophy, good in French composition and very good in the elements of the sciences."

He was a serious-minded youth, with his thoughts set on high things, and with an unlimited capacity for hard work and a grim determination to succeed. Withal, he was an unselfish youth fond of his family and of his friends. He writes to Chappuis in 1842, "My greatest pleasure is to get letters from you and my own people, so write often, and may your letters be very long ones." He writes to his sisters in 1840 from Besançon: "Work and love each other. When one has learned to work he can not live without it. Besides, everything in the world depends upon it." In another early letter he says: "Almost always hard work leads to success. Three things go hand in hand: will-power, labor, success." To these should be added good judgment, and he was full of that.

At this time he was simple, grave, almost timid, but with a flame of enthusiasm, a worshipper then as ever of noble impulses and great men. You remember what he said to the students at Edinburgh when he was old and famous, and what he wrote of Lavoisier when he was young!

Some early opinions of him are interesting. Pouillet wrote: "I do not know a young man more honest, more laborious and more capable."

Biot said "He illuminates everything he touches." Biot also wrote to the father describing the son as "so good and so distinguished" and adding: "It is the greatest pleasure I can experience in my extreme old age to see young men of talent, active and industrious, who seek to advance in a scientific career, by real labors, solid, long-time followed, and not by miserable intrigues."

Senarmont wrote in 1851: "If he keeps on, what he has found is only the beginning of what he will discover."

The great chemist, J. B. Dumas, said in 1865 when he asked Pasteur to undertake the investigation of the silkworm plague:

"I do not know any one at the present time who represents better than you the spirit of Lavoisier and his method."

The Man

Tremendous will power and an insatiable desire for knowledge and especially new knowledge to be obtained by experiment characterized boy and man. Added to this he had intuition and scientific imagination of a high order. Philosophy seemed to him a chimèra. He formed innumerable hypotheses to explain what he saw about him, but only to try these out one after another in the crucible of experiment. Hypotheses come to us in armfuls, he said, but they are of no value unless verified. As Duclaux has said, Pasteur loved great horizons, knew how to discover them and to make himself a part of them. Now he was on the mountain top full of expansive joy and ready to combat the world when some crucial experiment had succeeded beyond his fondest expectation, and again he was in the valley of despondency when all his experiments had failed and the way appeared to be walled up. He observes of a year of failures, "Il faut être un peu fou pour entreprendre ce que j'ai entrepris." Duclaux gives us a striking picture of Pasteur at another such a time. After working for two years on the silkworm plague with varying success he comes in one day utterly dejected and throwing himself down in a chair announces, "We have failed. Everything must be done over. There are *two* diseases!" But discouragements were only temporary with this man. Obstacles, inanimate or human, only seemed to stir him to greater efforts. When he is working on a deep problem in crystallography and is perplexed we hear him cry: "I will work ten years on it if necessary." At another time of discouragement he says: "The essential thing is to do over again all our experiments." In Strasburg he writes to Chappuis: "The nights are too long. I prepare my lectures easily and I have five days a week for the laboratory." To Madam Pasteur, who sees too little of him, he says by way of compensation, "I will make you known to posterity." The woman's side of the case comes out humorously thirty years later in a letter to her daughter: "Your father is absorbed in his thoughts, talks little, sleeps little, rises at dawn, and in one word continues the life I began with him this day thirty-five years ago."

Nothing could draw him away from his researches, yet he did not skimp his teaching. He said, "If I neglect my preparation only a little I teach badly and am obscure."

Pasteur was a pioneer and he had the faults and virtues of the pioneer. He cut roads in every direction through vast territories.

He stands distinctly ahead of Robert Koch both in point of time and of genius and I say this without desiring to minimize in any way the great researches of the latter. Like Spinoza he was intoxicated with his work. "We must never be satisfied," he said, "with what we have already acquired."

He attends a great dinner of chemists given by Thénard in honor of Mitscherlich, who is visiting in Paris, and what he brings away of most value is that there is a factory in Germany where he can get once more the problematic racemic acid. Later he chases all over technical Germany and Austria seeking it. The results of his first three days in Leipsic in 1852 are recorded in a letter to his wife as follows: "Dear Marie: I can not await the end of my researches, but must write you again. Yet I have nothing to tell you except that I have not left the laboratory for three days and all I know of Leipsic is the street that leads from the hotel to the university. I come back at night, get my dinner and go to bed." This is how Pasteur saw Leipsic, but in Dresden he had to wait for a train, and another side of his nature came uppermost, for he says: "I have passed four remarkable hours in the galleries marking the pictures I like +, double +, triple +, and even quadruple +." It would be interesting to know what pictures he liked.

At another time he silences those who ask what practical application he expects from his discoveries with the words of Franklin to other "hard-headed, practical people"—"What use is a baby when it is first born?" If those who derided Franklin and his kite could see what has been done with electricity in modern times they would hide their heads, as would those who sneered at Pasteur.

To another who remarked to Pasteur, "It was a mere chance," he replied, "Chance favors only those who are prepared to take advantage of it."

Soliciting academicians for votes he calls "Un vilain métier."

Pasteur was a man of high ideals and his patience and his power of concentration were remarkable. The diligent in business shall stand before kings! Was ever a truer saying or ever a greater example of it than Pasteur? The curse of the world to-day is the desire to shirk honest, long-continued hard work. No man, no organization, no nation that follows this course, can succeed or endure very long—it is the path to individual and national decay, and a great many persons and groups of persons in various countries are now headed in that direction.

Pasteur hewed to the line through a long series of years and at the close of his life we hear him still saying, "Duty ceases only when the ability to labor is at an end."

He was a good son and brother, a good husband and father, a good friend and neighbor, a penetrating observer and persuasive lecturer, but he was much more, for, especially in the second half of his life, the interests of a sick and suffering humanity were always knocking at the door of his heart and making of him more and more a great philanthropist and humanitarian.

He was a veritable Æneas of filial piety. In 1865 he writes to his wife: "The poor grandfather is no more, and this morning we have placed him in his last resting place at the feet of the poor little Jane. I have thought all day of the goodness of my father. For 30 years I have been his constant and almost sole preoccupation. I owe everything to him. When I was young he withdrew me from bad companions, taught me to work and set before me the example of a very loyal and productive life. By the distinction of his spirit and his character this man was much above his position in life, if we judge things as the world judges. He was not deceived; he knew very well it is the man that honors the position and not the position that honors the man. You did not know him, my dear Marie, in the days when my mother and he toiled so laboriously for their dear children whom they loved so much, for me especially, whose books and months in college and pension at Besançon cost dear. I see him still, my poor father, instructing himself without ceasing in the leisure left from his manual labor, or designing and making wood carvings. He had a passion for knowledge and study. I have seen him studying grammars, pen in hand, comparing them and making notes to have at 40 and 50 what the hard fortune of his early years had refused him. But the books he loved best and read most were those that recalled the facts of the great imperial epoch which he had served in his time on the fields of battle and which had renewed society. Good-bye, my dear Marie, and my dear children. We shall speak often of the grandfather of Arbois."

To a young man of good parts who complained of ill treatment he writes: "The discouragement in your last letter is not worthy of a scientific man. Do your duty as well as you can and don't worry about the rest."

Pasteur was a religious man, born and bred in the Roman Catholic communion and remaining in it all his life, but I have never discovered in him any great liking for ecclesiastics. Their methods were not his. He always insisted on the absolute freedom of the scientific man to investigate truth in every direction without reference to consequences, and untrammelled by any religious or philosophical notions. He said: "Three dominating ideas of an unselfish life are God, country and liberty (1883)."

Science, he said, deals with facts, secondary causes, phenomena.

Search for a first cause is not within the domain of science. He succeeded the indefatigable, great and good Littré in the Academie française, and speaking on Comptism he said:

"Beyond this starry vault, what is there? Other starry skies. So be it! And beyond? The human mind, urged by an invincible force, will never cease to ask: What is there beyond? It serves no purpose to reply: Beyond are limitless spaces and times and grandeurs. No one can understand these words. He who proclaims the existence of the infinite, and none can escape it, accumulates by this affirmation more of the supernatural than there is in all the miracles of all the religions for the notion of the infinite has this double character: It impresses itself on us and it is incomprehensible. When this notion takes hold of the understanding we can only bow down before it. The idea of God is a form of the idea of the infinite. So long as the mystery of the infinite shall weigh upon the human mind, temples of worship will be raised whether the god be called Brahma, Allah, Jehovah or Jesus, and on the floors of these temples, you will see men kneeling, prostrate, swallowed up in the thought of the infinite."[2]

To students he said: "Young men, . . . live in the serene peace of the laboratories and libraries. Ask yourself first: What have I done to repay my education? Then, as you advance: What have I done for my country? And finally, perhaps, you shall have the immense happiness of reflecting that you have contributed in some measure to the progress and well-being of humanity. But whether your efforts are more or less favored by life you should have the right to say as you approach the great end: I have done what I could."[3]

Science went begging in the France of the forties as in every other country. Even the greatest men had meager incomes and mere hovels to work in and yet they produced many and brilliant results. Pasteur received room and board and 300 francs per year for his first work as a teacher and thought that too much money for the services performed. At Strasbourg he bought laboratory equipment out of his own purse (prize money). Even after eight years of teaching in Strasbourg and Lille when he came to be professor and sub-director of science studies in his beloved Normal School in Paris he had no assistant, very little money for apparatus and chemicals, less than the least well-equipped institution in this country can boast, and quarters only to be compared to a stable or a shanty, but he had what many of us lack, namely, an indomitable will and the determination to make the most and the

[2] Academy Discourse, April 27, 1882.
[3] *Jubilee Discourse*, December 27, 1893.

best of everything. Not a little of his early success was due to the kind of friends he made, but he was able to win and keep the friendship of the greatest scientific men of France, such men as Dumas, Biot, Balard, Saint Claire Deville, Claude Bernard and Thénard, only because he was himself genuine, had already made notable discoveries, and gave promise of a brilliant future. From the time he entered the Normal School as student in 1843 until his death in 1895 his life was a long series of triumphs, of splendid discoveries, and of private and public gratulation. All doors were open to him, academies elected him, royal societies honored him, universities invited him to lecture, medical societies gave him honorary membership, surgeons and heads of technical industries consulted him, the Emperor sent for him, the Parliament granted him an annuity for life in recognition of his public services and later doubled it, but through it all he remained the same simple, unaffected, honest man searching for the truth in every direction and especially eager to discover and destroy the causes of the great human and animal plagues which he believed to be due to various undiscovered ferments.

Pasteur has been accused of being callous to the infliction of pain and has been called an "archdevil" by some of our friends and neighbors who love cats and lap-dogs more than they love children and a suffering humanity. But beyond all doubt, like his father, he had a tender heart for all who suffer; and who can measure the alleviation of pain his researches have brought to mankind and to some of those lower forms that have no voice but a cry! A hundred anecdotes show his sympathy both for men and animals. I have space for but one. To-day, death from puerperal fever is the rarest of diseases. In Pasteur's time it was the commonest. Every third mother in some places might expect to die in childbirth. Imagine, if you can, the lecture room of the Academy of Medicine full of learned doctors peering over their spectacles in amazement at his temerity as he interrupted and contradicted one of their greatest, saying, "the cause of this disease is not at all what you think it is, but it is a germ. I have seen it, and this is its shape [drawing it] and you carry the disease from patient to patient on your hands and on your instruments." What should a young chemist know about puerperal fever! yet he was right and they were wrong, and in his vision he saw a multitude of mothers needlessly sacrificed who could be saved and now are saved. Should not the women of to-day rise up and call him blessed? Yea, verily! Suppose he destroyed some experimental animals in learning this great truth. We destroy multitudes of animals, and often very painfully, merely to wear feathers on our hats, furs on our backs and gloves on our hands or to eat more

flesh than is good for us, and often, too, for the mere pleasure of the killing, as in a great deal of hunting and fishing; but he and his kind, down to this very day, only for a large service to humanity and to the domestic animals. Small wonder, too, that he was earnest almost to rudeness before these doctors, as frequently in other discussions, when he saw the truth set aside and error exalted. He was a rude and fierce debater, and one polemic followed another in quick succession. The love of truth compelled him, a certain inheritance drove him. To opponents of his crystallographical views he shouts: "If you know these things, where is your conscience? If you do not know them, why do you meddle with them?" And afterwards, as they went home together, Duclaux made him laugh by saying: "I half expected to see you throw the models[4] at their heads." Again he says of Pouchet and his followers: "I have the truth on my side. They do not know how to experiment. It is not an easy art."

I could multiply such examples. There are many in Duclaux's wonderful book, "Pasteur, the History of a Mind," and in Rene Vallery-Radot's "Life of Pasteur," both now available in English. Pasteur Vallery-Radot, the grandson, is also editing a definitive edition of the writings of Pasteur, something long needed.

In addition I have only time to touch for a few moments on some of the high lights in his scientific progress. Pasteur began as a chemist, he finished his career as a human pathologist. Along the way he was a physicist, a mineralogist, a microscopist, a histologist, a mycologist, a bacteriologist and a protozoologist. He studied all kinds of fermentations that came to his hand, alcoholic, acetic, lactic, succinic, butyric, and types of microorganisms (aërobic or anaërobic) that are involved. Milk, beer, wine, vinegar and the decay of meats, fruits and vegetables interested him by turns. He and his students revolutionized the dairy industry, beer brewing, wine-making and the production of vinegar. The study of the diseases of silkworms led him to anthrax and chicken cholera and wound-infections, and these led him to rabies; and all these studies led him to vaccines and sterilization by heat and by germicides and gave him wonderful glimpses of a promised land which we have begun to enter into and occupy, but the wonder and wealth of which are still only imperfectly realized, especially as related to the public health. In his book on "Diseases of Silkworms" (Tome I, page 99), Pasteur says: "It is within man's power to rid the world of parasitic diseases, if, as I believe, the doctrine of spontaneous generation is false." So vast was his dream! We shall never quite accomplish the extinction of any

[4] Gigantic models of his crystals.

parasitic disease, there are so many unsuspected carriers of disease and other uncontrollable factors involved (inheritance, for example,) but we have already reduced some of the great scourges of mankind to small proportions, *e. g.*, smallpox and yellow fever, and many other similar triumphs must follow when the public are willing to trust the scientific man and put into practice what he already knows. Pasteur glimpsed all this.

Much of his work, of course, was done by means of assistants but he was the man at the helm. We are filled with amazement at what he accomplished, especially when we remember his handicap. His mother died of apoplexy and he suffered a hemiplegia of the left side when he was in the midst of his silkworm studies in 1868, so that for some days his life was despaired of but gradually he recovered and was able to limp about, undismayed and thrice determined. Indeed, all his greatest work was done in the 27 years that followed the first paralytic stroke which would have been a knockout blow for most men. He is a great example of will-power directed to noble ends.

Finally, when so many things are being said against France, it is well to recall not only what she has endured but what she has given to the world. To me she is to-day intellectually and morally and scientifically the greatest nation on the continent of Europe and I resent anything said against her. Rather let us praise her fortitude, glory in her great men and join hands with her whenever we can.

FURRED FOREST PLANTERS

By Dr. J. V. HOFMANN

WIND RIVER FOREST EXPERIMENT STATION

THE forester must provide for the utilization of the forest and, at the same time, insure its perpetuity. The manifold factors which influence regeneration must be known and their direct or indirect effects considered in any attempt to control or insure regeneration. These factors may be climatic, edaphic, or biotic. Unfortunately, the biotic factors have generally been classed among the destructive factors, and the rodents and some species of the bird population of the forest have come to be considered nuisances and their extermination ruthlessly advocated. Those whose forest visits and camp life have been made miserable by the aggressive companionable natures of these woodland inhabitants have a personal grudge to settle, and those who have seen their favorite plantings of seed serve as a festive celebration at which, apparently, no race or color lines were drawn can tolerate no leniency in their sentence. This "hard-boiled" lot is joined by the group who have made their plans to collect forest tree seeds and then found the program of the rodents was along the same line at a considerably earlier date.

This antagonistic feeling has become so deep seated that the rodent is condemned without the committal of an overt act. Here the old adage that "a little knowledge is a dangerous thing" should be revived and the rodent given a fair and impartial trial for his life.

The gray squirrel is condemned in California because it destroys practically all of the sugar pine seed. This fact is easily checked and need not be questioned. Yet reproduction of sugar pine has become established there in the past as have also the present stands of timber of this species. These stands were very probably planted by the squirrels, and the reason that reproduction fails in these regions now may be due to the unbalanced relation of the number of squirrels and amount of seed produced. It is a well-known fact that rodents will cache supplies which they depend upon for food during the year; consequently, for reproduction, there must be a surplus of seed over and above the food supply of the rodent population.

A change of equilibrium may be brought about by the destruction of the rodent's enemies, such as hawks, owls, weasels, snakes, bobcats, badgers, skunks, minks, coyotes, etc., and also by reducing the number of seed trees to the point where the supply of seed does not exceed the demand.

The distribution of the black walnut is attributed almost entirely to rodents. The reproduction from cached nuts in open tufts of grass or near logs and stumps are direct evidence of rodent activities. The same condition is evident with other nut-bearing species, such as the oaks, hickory, etc. Where seedling of those species occur on slopes above the parent tree and in localities where there is no immediate other source of seed, it is plain that the rodent has contributed a part of his stored food to the cause of forestry.

Distribution of western yellow pine in Montana, Idaho and Oregon has been found to be due in many instances to seed cached by rodents. Mice are very instrumental in this work because they cache individual seeds rather than cones. Chipmunks do this also by selecting a different place for caching each mouthful of seed which they gather. In this manner the yellow pine seedlings become established on the grassy slopes where seed that merely falls on the surface would not have a chance for establishment.

Direct seeding of coniferous species has been practically abandoned in the National Forests, due almost exclusively to the destruction of the seed by rodents. In all experiments the results have shown that the number of rodents present on the areas would consume many times the amount of seed sown. Poisoning has been partially successful on local areas but has not been found practical. On the Pike National Forest in Colorado caches of poisoned grain of one pound and less were found. This shows the instinct for hoarding even before a full meal has been eaten.

In the Douglas fir forests of the Pacific Northwest rodent life is very abundant and is an important factor in forest management. Research has determined that the even-aged stands of reproduction, which occur over large areas after forest fires, are due to seed which was stored in the forest floor before the fire and retained its viability through the fire. That this seed storage is in a large part due to the rodents is emphasized by the occurrence of reproduction in groups from caches in some instances, and most strikingly by the occurrence of stands having up to 40,000 seedlings per acre where all the litter and duff has been consumed by fire and no seed trees are left on the area.

This was found to be the condition on the Cispus burn of June, 1918, when examined in July and August, 1919. This fire covered

about 150 square miles north and west of Mount Adams on the Rainier National Forest. The fire occurred so early in the season that there was no possibility of seed production in 1918. The area burned over included largely a 1902 burn which had restocked generally with a good stand of reproduction.

In the 1902 fire some groups of trees escaped which remained green until the fire of 1918. Where these isolated groups of trees were killed in 1918 there was no source of seed left except that which had been produced in 1917 or earlier by these trees. Good reproduction occurred under such groups of killed trees, but not out from them, even where the duff was completely burned and the surface of the light pumice soil showed that it had been severely burned. Seed which had merely fallen from the trees would have had no chance of escaping such a fire because it would have been in the litter and humus. The present stands of young growth occurring on these areas, therefore, must be credited almost entirely to rodent-cached seed before the fire which was planted deep enough to escape the fire. The planting of seed in the mineral soil is also a decided advantage in the establishment of the seedlings in such localities.

During the dry season the surface soil dries out to a point where the water content is less than the requirement of seedlings; consequently, it is essential that the seedlings form deep root systems before the dry season. Seedlings in the mineral soil have a better chance to develop deep root system than those that germinate on top of the duff.

During the caching of seed a large per cent. of the crop is eaten, and in years of light seed production there is probably very little left, excepting such caches as can not again be located.

Tests have shown that humus is among the most effective mediums for disguising the odor of seed. When seed was covered with one and one half inches or more of duff mice would generally miss it, although in mineral soil they would find it at the same depth. This would have direct application in caches which were left beneath the duff in the fall and all traces of which had been obliterated by snow and rain before spring.

Rodent activity often accounts for the lack of reproduction from scattered single seed trees. The failure of seeding from single seed trees has been noted in many instances in the Douglas fir region, also on the Pike National Forest and Minnesota National Forest. The white-footed mouse of the Cascade region has been found to consume 300 Douglas fir seeds per day, and the chipmunk 600 seeds per day. From these figures it is readily seen that only a few rodents would consume the entire crop of seed from

scattered seed trees where the average seed production is 20,000 seeds per tree as is true in this region.

Rodents, apparently, do not carry seed to any great distance; consequently, they can not be depended upon to restock areas which are cut over or burned at distances more than about 300 feet from seed trees. Single instances of distribution to great distances by birds or animals no doubt occur.

The close relationship of the animal life of the forest and the forest itself brings out clearly the benefits which each derives from the other, and also emphasizes the broad general interdependence, while the problems of control and damage are local and specific.

The benefits of rodent activity in the regeneration of forests are usually indirect and not as noticeable as the damage, and too often judgment is passed where only the destructiveness of the rodents is considered. These factors should be carefully investigated and weighed against one another before the rodent is pronounced a nuisance or a benefactor in any forest region.

While the rodent has been considered the enemy of the forester, it is evident that he has played a large constructive part in the success of the natural regeneration of forests.

CERTAIN ECONOMIC REACTIONS OF THE WAR[1]

By Professor JAMES MAVOR
UNIVERSITY OF TORONTO

STUDY of reactions of a specific force upon any system involves preliminary estimates of the character of the system before the force in question began to act upon it. The conditions of this address render a serious survey impracticable. I shall therefore assume a general knowledge of the character of the European and American economic systems as they were when the outbreak of war occurred; but I wish to direct attention briefly to the following:

Growth and Movements of Population

The population of Europe grew during the past fifty years at the average rate of about one per cent. per annum; towards the close of the period, the rate exhibited a tendency to increase. The rate of increase of urban population was much greater than that of rural population, even in countries predominantly agricultural like Denmark. Emigration from some countries, notably Germany, declined in the fourth quarter of the nineteenth century. Emigration from Austria and Italy declined during the first half of the second decade of the twentieth.

Periodical migrations assumed large proportions. Thus, about a million Italian *contadini* customarily left Piedmont and Lombardy every year for the South of France to assist in the harvest, and a similar number of Russian laborers customarily left Russia for Germany for the same purpose, both large groups returning when the harvests in the respective countries were over. Within the countries mentioned, Germany, France, Russia and Italy, there was also extensive temporary migration during harvest time.

Development of Transportation

While movements of population do not necessarily depend upon transportation facilities, for many of the great migrations of history have been conducted with the most slender means, railways and steamships have facilitated these movements, especially from

[1] Address of the vice-president and chairman of the Section of the Social and Economic Sciences, American Association for the Advancement of Science, Boston, December, 1922.

Europe to America. The pressure of population in Austria, for example, and particularly in Galicia, was materially relieved by the cheap zone tariff on the Austrian railways and by extremely low competitive fares on connecting railway lines and on ocean steamers. It was possible for Galician peasants to emigrate, even although they possessed almost no capital. Their emigration relieved the pressure of population, raised the wages and standard of comfort of those who remained and contributed to reduction of land rents. In Southern Italy emigration had the same consequences.

Demand for Commodities

Increase of urban population in every country in Europe in greater proportion than the increase of rural population involved increased industrialization, and although wages were naturally influenced by the growing stream of workers, the means of the latter were enhanced and therefore their capacity for consumption became greater and more varied than it had been in the self-contained rural villages. Increased profits arising from increased production rendered possible by the influx of laborers enlarged the means of the middle classes and enabled them to live at a higher standard of comfort than that to which they were formerly habituated. Thus manufactures, imports and exports were subject to rapid increase during the fifty years immediately preceding the war.

The industrialization of Europe meant increasing dependence for food upon less highly industrialized regions. Imports of wheat, rice, tea, coffee, sheep and cattle from North and South America, China, Japan, India, Australasia and North and South Africa formed a large proportion of the foodstuffs of Europe.

An industrial community requires not alone food, but also raw materials. The more complex industry becomes the more widely extended must be the area from which these materials are drawn. Many of the raw materials of modern industry are drawn like rubber and cotton from tropical or semi-tropical countries, or like timber from regions remote from the seat of the industry. If the supply of raw materials fails for any reason, unemployment ensues and a strain is imposed upon the social system. The danger of such a situation was not unforeseen; *e. g.*, the British government appointed in 1903 a commission on food supply in time of war. This commission published in 1905 a series of valuable reports which showed how narrow a margin of safety existed in a highly industrialized country like Great Britain. The commission found that in 1870 the proportion of imports to consumption of wheat was 1.4 per cent.; in 1880, 8 per cent., and in 1900, 25 per cent. It

was also found that in only four years out of eleven years immediately preceding 1904 was there always a sufficient supply of wheat in warehouses at the ports to provide for two and a half weeks consumption. Germany and France were not industrialized to the same extent as Great Britain, yet they were vulnerable in the same way. In these countries, also, the urban population had grown at the expense of the rural, the production of foodstuffs had relatively declined, imported foodstuffs and raw materials were composing increasing proportions of the total consumption.

Even in times of peace, the margin of safety was small. Any disturbance from the normal flow of commodities produced hardship. Thus in Great Britain there was a sharp scarcity of wheat in consequence of the Leiter operations in wheat in the Chicago market in 1898, the supply being affected from June to August of that year.

Movements of Prices

The characteristic of the period of about seventy years from the close of the Napoleonic Wars till the middle eighties was decline of prices. The purchasing power of gold increased steadily, notwithstanding the increase in the production of gold which began in 1849.

From the middle eighties until 1914, a period of about thirty years, the purchasing power of gold diminished, prices became higher owing in general to two concurrent causes—the increase of demand for commodities without proportionate increase in production and the increase in the amount of gold available for purposes of circulation.

The Currency Systems

Increase in the production of gold from 1886 enabled European governments and European bankers to accumulate gold reserves to a much greater aggregate amount than had previously been possible. The currencies of Russia, Austria and Italy had been depreciated on the foreign exchanges for many years. By the beginning of the twentieth century the currencies of all these countries reached the par of exchange. Great Britain has for a long period adopted the policy of merchandizing gold rather than storing it. Her bankers have looked upon dealing in gold as more profitable than keeping unnecessarily large stocks of it. France has for long adopted the policy of warehousing gold and of checking exportation of it by imposing a premium when gold is withdrawn for export. The other continental countries in general followed the example of France. The possession of large gold reserves against their issues of paper currency gave the cur-

rency of all of the European countries stability and the fluctuations in exchange were due entirely to the demand and supply of the instruments of exchange from day to day. These fluctuations were not caused by increase in issues of paper, because these increases, when they did take place in obedience to public demand for them, were based upon a sufficient gold reserve to remove any doubt as to their convertibility into gold if the holder so desired.

Wages and Conditions of Labor

The influx of laborers into the towns which was going on with accelerating speed towards the close of the period tended to keep wages down in spite of the demand for commodities. In those countries in which the cities were not readily susceptible of rapid expansion, the conditions under which the laborers lived were often deplorable. Even in those countries where public authority, private enterprise or private benevolence grappled with the question, there was much overcrowding in the industrial centers and a very definite tendency towards deterioration of the industrial population. Newcomers did not easily adjust themselves to the life of the city, and even if they could have done so the conditions of the city were not favorable for rapid absorption of increasing numbers. By the close of the period real wages had in general increased with the increase in the production of commodities and some of the causes of fluctuation of employment had been brought under control by skilful administration of industrial enterprises. Limitation of the hours of labor of women and children by law had been provided by the earlier factory acts; but the principle had not been extended to adult male labor. Demands for an eight-hour day in certain industries began to be formulated, especially in England. Later these demands became almost universal throughout Europe. So also demands for the institution of a legal minimum wage were advanced in many countries. By 1914 these projects were being urgently pressed by the representatives of labor. It appeared as though the industrial world was returning to the restrictive conditions of the 18th century.

Labor Movements

By labor movements, I mean the grouping of laborers in trade unions or other forms of combination for the purpose of making collective demands or arriving at collective bargains. For the first half of the past hundred years, combinations of laborers were forbidden or discouraged in almost all countries. So also were combinations of capitalists. Both were forbidden on the ground that society as a whole might suffer through combined action resulting in what was regarded as artificial conditions in which

wages and prices were higher than they would have been had there been no such combination. About fifty years ago the legal obstacles which were in the way of joint stock enterprises were removed and although some of the restrictions upon the employment of capital remained upon the statute books of most countries, even these fell into desuetude. Legal restrictions upon labor combinations remained longer, but in the third quarter of the nineteenth century they disappeared in England. Labor movements in all countries were given at this time a great impetus. Almost all these movements led to the emergence of two factions—one faction insisting that in order to improve their condition it was necessary for the laborers to influence or even to control legislative bodies and governments, and the other that the laborers should not concern themselves with politics but should devote themselves to the promotion of economic means for the improvement of their position. The first faction leaned towards collectivism or towards a proletarian government which might or might not be collectivist, and the second towards trade unionism, towards strikes or towards syndicalism or even towards *"l'action direct."*

Movements of Capital

By this I do not mean a capitalist movement as opposed to a labor movement, but the movements of capital from one country to another and also certain changes in the manipulation of capital which characterized the period immediately before the war. The transference of capital on a very large scale from Great Britain and France to other countries had been going on for about fifty years up till 1914. I need not trouble you with statistics of these investments; they can be readily obtained from the customary authorities. The field for investment in Great Britain and France, though large, is, nevertheless, comparatively limited, and the former especially was able to conduct her world-wide trade very largely because of her world-wide investments. Great Britain supplied the greater part of the capital by means of which the American railway system was recreated after the Civil War and at the same time she was building railways all over Europe. Since then many of the high buildings in the United States cities have been financed by the British insurance companies and other investing institutions. France has also played a conspicuous rôle in furnishing capital for foreign enterprises. The annual increments in the capital available in the market for borrowing which these two great saving countries supplied were to be counted upon and were counted upon in the finances of all countries. Germany also had for some years been accumulating foreign investments. During the whole period of the history of the United States up till 1914

they were borrowers, and there were therefore at that date immense sums of foreign money invested in them.

Changes in the manipulation of capital occurred chiefly in connection with joint-stock enterprise. The corporation took the place of the individual employer, and combinations of employers were followed by combinations of corporations. The formation of very large corporations appeared at times to be determined by economic necessity. Such combinations were not confined to the United States. Trusts in England and *cartels* in Germany were formed in the same manner and for the same objects. So long as these corporations did not come into direct relations with the general public, their formation and the methods with which they conducted their business did not excite any public apprehension. But those corporations, such as the railroads, street railways, electric power and lighting companies and the like, whose services are utilized by every one, were peculiarly open to public criticism and attack. The furore against corporations attained its full vigor about the end of the nineteenth century. Then began reversion to the legislation against combinations of capital which I have noticed as belonging to an earlier time.

Mobility of Property in Land

In the United States, especially during the past fifty years, property in land has been relatively mobile. Land changes hands with frequency. In Europe, this was not generally the case. The possession of landed property was, even in the case of small holdings, hereditary. Ownership of land had a certain honorific value. The landowner enjoyed a social distinction denied by the people in general to those who were not landowners. This condition existed all over Europe, but it was especially observable in Eastern Europe and chiefly in Russia, where the landowners occupied a privileged position under the sanction of the law.

Public Opinion Regarding the Limits of Functions of the State

Public opinion on the functions of the state has been subject to great variations during the past hundred years. At the beginning of the period, the state was looked upon as a necessary evil—an institution which was to be tolerated in the exercise of those functions which could be exercised by the state alone; but every attempted encroachment of the state upon private liberties was to be resisted. The governments of even so-called democratic states were often offenders against liberty; therefore, they should be confined strictly to the sphere which indisputably belonged to them.

The Germans were really the first to take a different view of the state and its functions. This view probably had its real origin in the conception of the unity of the state and in the still earlier

unity of the tribe. The Germans were driven to this view by the poverty and depression of Germany in the period immediately succeeding the Napoleonic wars. The German philosophers of that time, at whose head stood Fichte, saw that the people would never emerge from the pit into which they had fallen without organized means. There was an existing organ whose function might fairly be regarded as including the ordering of the whole life of the people in such a way as to make them economically prosperous and politically stable and powerful. This rudimentary view was developed by Hegel into a state philosophy in which the state became personified into a kind of beneficent Providence, an economist in the sense of energetic housekeeper who managed everything for everybody, who was always at everyone's call for services of all kinds. Undoubtedly for depressed Germany such an idea had its validity and its fruitfulness. By means of it the Germans rose from the dust and became a great and powerful people. But the state is not merely an abstraction; it is a concrete reality, visible in the persons of those who compose the government, and the resources these persons have at their disposal are entrusted to them by the people to whom these resources belong. It is human to err, and governments of states are human. Thus, at times the tangible agents of state action were not always beneficent and efficient housekeepers but often quite otherwise. The state, instead of corresponding to the ideal of Fichte or even to the ideal of Hegel, became an autocratic monster in whose actions it was difficult to discern any of the moral purposes which it was supposed to personify.

England was slow to adopt the German idea of the state as it developed in Germany about the middle of the nineteenth century. Yet by the year 1870 her legislation began to show evidences of change. This change did not pass unnoticed. Herbert Spencer, *e, g.*, denounced it as making for a "*new slavery.*" But the extension of the franchise in 1832, 1866 and 1889 had resulted in a new electorate which was little used to abstract discussion about the limits of state functions. They had been endowed with a voice in the government of the state and they intended to ask for what they wanted and they meant to get it. They were not interested in the origins of doctrines; but they were interested in utilizing the machinery of the state for any purpose for which that machinery might be used. There thus began an era of state and municipal enterprise—an era in which what were called public services began to be rendered by state and municipal officials. This era may be said to have extended in Great Britain from about 1870 till about 1903, a period of a generation. Although the enterprises upon which the state and municipality embarked during that gen-

eration have been in general retained, few enterprises embodying any new principle were entered upon by either after that time. The furore for public ownership began later in the United States and lasted longer.

Public Indebtedness and Taxation

The most important and inevitable result of the expansion of state and municipal activities were the expansion of the public debt and of the taxation of the public. All countries which adopted the view of the functions of the state which I have described, either as a reasoned theory or as a ground of practical action, added to the obligations of the state, multiplied the state functionaries and added to the taxation imposed upon the people. Several important consequences followed the adoption of this policy. Fixed capital, that is, capital invested in factories, houses, machinery and the like, was so heavily taxed in Great Britain, for example, that the net returns yielded relatively smaller amounts for foreign investment and so diminished the flow of capital to foreign countries, not absolutely but relatively. Circulating capital, if heavily taxed in one country, tended to flow to another country, where it was not so heavily taxed. Another consequence was that the investment of capital in fixed forms was discouraged, and there thus developed a scarcity of house accommodation and much overcrowding in the growing cities. As regards debt, the competition of public securities in increasing volume with other securities in the market depressed all securities and raised the rate of interest. There was also manifest a tendency towards incurring floating debt in large amounts.

It is now my task to examine with unavoidable brevity the reactions of the war and the peace in these various categories.

Population and Its Movements

During the war the growth of population was affected by the enlistment of all the able-bodied men of practically the whole of Europe. Men of military age were conscripted and were not permitted to leave their countries. Migration was suspended. The losses of the war diminished the population of every country temporarily and prevented growth that would otherwise have taken place. Experience has shown that after great wars populations in general exhibit resilience. They soon repair in a numerical sense the damage. The actual loss of men will thus ere long disappear from census statistics. Yet the loss of the flower of the youth of France, Great Britain and Germany will not soon be overcome. In some districts in all of these countries the losses were so great that a generation or more may elapse before the numbers of the people

through natural growth can recover. There are villages in Scotland in which at the close of the war there were left only aged men, women and infants. The mature able-bodied male population had been wiped out.

In the theatre of war there were important movements of population. Holland and Switzerland were both crowded with French and Belgian and later with Russian refugees. The Belgians and the French population fled from their homes on the advance of the German armies, and the Germans in East Prussia poured into Eastern Germany when the Russians advanced through the Mazurian marshes in the early days of the war. So also the peasants of the plains of Galicia and Bukowina and later those of the Carpathian valleys fled before the Russian advance, abandoning their houses and carrying with them their slender portable belongings. The flight of the Serbian peasants through the mountains of Northern Macedonia when the Austrians advanced into Serbia was one of the most tragic episodes in Balkan history. When Roumania was defeated, Roumanians poured into southern Russia, and poured back again when the Bolshevik revolution compelled them for a second time to become refugees. The Italian farmers were driven back by the advance of the Austrians on the Piave.

All these movements have had important economic consequences for the people concerned in them. Their life was torn up by the roots. Families were disrupted and the products of their own industry and that of their ancestors destroyed by shot and shell. The devastation of their fields has meant the destruction of the fruit of centuries of toil. The fixed agricultural capital and the fixed capital in dwellings together represent the product of practically infinite labor.

Apart from these movements of population as direct consequence of military operations, while the war resulted in the mobilization of armies the population in general became less mobile. Traveling was expensive and troublesome, the movements of troops taking precedence of everything, and the movements of civilians were embarrassed by passport regulations. After the war the treaties of peace altered the map of Europe, cut large areas from Germany, Austria, Hungary and Turkey; in some cases added these areas to already existing countries, as Alsace and Lorraine to France and a part of Schleswig-Holstein to Denmark, or created new countries out of fragments taken from the defeated nations—as Czechoslovakia, compounded of fragments taken from Germany, Austria and Hungary, and Jugo-Slavia, composed of former Balkan states. In each case various considerations impelled the makers of treaties to include this or that people with this or that

nation. It was inevitable that some should be dissatisfied with this arbitrary method of settling the nationality of individuals. Many, therefore, migrated from the region in which they had resided and went to places where they might be associated with their own people. More than a million Germans left Alsace-Lorraine and Upper Silesia for Germany when these regions were detached from the German Reich. Other migrations accompanied by great hardships occurred in Turkey. Deportation of Armenians and expulsion of Greeks, the consequence of the adoption of a new nationalist policy by the Young Turks, of which the slogan is "Turkey for the Mohammedan Turks," led to migrations of hundreds of thousands of people, accompanied by loss of life and sacrifice of property. These movements are apparently not yet over. Greeks are still leaving Turkey and Turks are still leaving western Thrace.

The disorganization of the economic life of these regions is still in progress. Since practically the whole of the trade as well as much of the industry and agriculture of Asia Minor have been carried on by the Greeks and since the Turk is a landed proprietor pure and simple, the economic ruin of Asia Minor is certain, unless, adopting the plan of earlier ages, by means of incessant warfare or by means of reorganization of the African slave trade, which had been put down by Great Britain, the Turk is able to secure the number of slaves necessary to ensure production.

The reaction of the war upon population within the theatre of war has been in short the pulverization of it, its economic life has been destroyed and the people have been dispersed.

Revolution has had a similar influence. The Russian Revolution, owing to its special character, has resulted in the flight of about two million Russians. Of these some have gone to Turkey, where large numbers found a refuge in Constantinople, to Japan, Italy, France, England and America. The flight of so large a number of people has compromised the economic life of Russia and has contributed with other causes to altered character of industry and trade in that country.

In consequence of all these movements, the margin between the birth rate and the death rate, which, apart from migrations, constitutes the natural increase of the population in any area was disturbed in all the European countries.

Transportation

Ocean transportation was paralyzed by the war, partly through the commandeering of shipping for military purposes, partly through blockade, and partly through the losses from the U-boat campaign. On land the railways—where these were in the hands of joint-stock companies—were taken over by the governments. The

railways of all countries were taxed to the utmost in the conveyance of troops and munitions. During the war, shipbuilding went on with unprecedented speed, new railways were built and new telegraph and telephone lines were erected. After the United States went into the war, practically the whole telephone system of France was reorganized by the American Telephone and Telegraph Company.

As the event proved, adequate importance had not been attached to the possibility of an unrestricted submarine campaign against mercantile shipping. The adoption of the U-boat policy by Germany was not carried unanimously. On the contrary, in general the German statesmen are understood to have been opposed to it on account of the political effect.

When the war began the British government brought into operation at once a system of insurance against war risks which had been long before prepared for the contingency of war with a maritime power. The subject had been fully discussed in 1904-5 and a plan was shortly afterwards devised.

In its earlier phases, the submarine inflicted great damage upon shipping and compromised the carrying trade, especially of food and passengers. The conveyance of troops and munitions was never seriously menaced. In time the menace of the U-boat was diminished by successful detection and sinking of large numbers of boats, so that the provision of crews became almost impossible. The last U-boat to be sunk at the entrance to Scapa Flow was found to have been manned entirely by naval officers who had volunteered for a service which ordinary seamen looked upon as involving certain destruction.

When the German mercantile navy was handed over to the Allies, the amount of the world's shipping had somewhat diminished, but the ocean commerce had diminished in greater proportion. The shipping companies entered into agreements to maintain freights by laying up ships. Building was arrested. At the present moment (December, 1922) the shipbuilding yards of Stettin and Hamburg are filled with ships in course of construction. It can not thus be long before a new German mercantile marine will challenge the Allied lines in all neutral ports as well as in those Allied ports which may be open to them. Freights will then inevitably decline. Great Britain took over the railways on certain terms from the companies and handed them back after the war was over; so did the United States. The German railways had for the most part belonged to the constituent states of the German Empire. When they were taken over by the Imperial Government, that government assumed all obligations attached to them.

The several states were both unable and unwilling to reassume the burden and the railways thus remain in the hands of the Reich.

Demand for Commodities

On the outbreak of war there was a sharp decline in demand for commodities in general consumption. The urgent demand was for ready money, and goods and stocks were sold for what they would fetch. This phase soon passed over. The chief effect of the war was the change in the commodities which were demanded—cloth for uniforms, rifles, guns of large calibre, unheard-of quantities of shot and shell and food for the armies. These were the commodities in urgent, increasing and eventually enormous demand. The greater part of the supply corresponding to the demand it was the task of Great Britain to organize. From the beginning large amounts of ammunition were manufactured by the United States, Canada and Japan, but the bulk of the supplies of uniforms, arms and munitions for the British Army, as well as a large part of those for the French and Italian armies and even for those of the United States, was supplied by Great Britain. The demand was so urgent that extremely high wages were paid, notwithstanding the fact that the working class was recruited by large numbers of women and by men not of military age who did not belong to the ranks of manual labor. Even so, specially skilled men had to be withdrawn from the trenches in order that those who remained might be supplied with munitions. During the final year of the war the production of military stores was overwhelming.

In order to produce this result, practically every industrial establishment in Great Britain had to be reorganized. This was not the task of the government or its officials; it was the task of the heads of businesses. On the outbreak of the war the British government set aside a large sum of money for the purpose of facilitating the transition of industry from a state of peace to a state of war. This amount was lent to the industrial establishments and repaid by them out of the profits of war contracts. Then, when the war was over, these industries had to be once more reorganized for the purposes of peace after four years of war. The former channels of trade had to be reopened, manufacture of customary products had to be resumed.

What occurred in Great Britain occurred in a more or less similar fashion in all the belligerent countries. New factories for the production of war material were built in Germany and Austria as well as in England, France and the United States.

Demand for the necessaries of life became acute in all the countries affected either by the British blockade or the German

submarine campaign. The former was by far the more effective. It rendered sea-borne traffic to Austria practically impossible and it rendered sea-borne traffic to Germany very difficult, although through Sweden a certain amount of traffic was sustained throughout the war.

The fact that every country in proportion as it was industrialized was compelled to import increasing quantities of food led to the widespread belief that a modern war must be of short duration because no country could long survive a blockade. In this belief, the endurance of mankind under exceptional circumstances was unduly underestimated. In all of the belligerent countries there was shortage of food. The fighting forces of the Allies, with the exception of those of Russia, were throughout well supplied. The fighting forces of Germany and Austria were also well supplied. In all of these countries, however, the civilian population suffered hardship, increasing in gravity as the course of the war lengthened. Certain commodities which have become necessaries of life, such as fats, sugar, etc., were latterly almost unobtainable in Central Europe. In all countries rationing systems were adopted.

Demand for commodities as it existed in 1913 has by no means been resumed. The purchasing power of the people has been diminished (a) in Western and Central Europe by taxation; (b) in Eastern Europe by social revolution; (c) in America by the decline in relative value of agricultural products.

(a) While the purchasing power of the people has been curtailed by increased taxation, the revenue represented by this taxation, in so far as it would have been expended by the people, had they had the disposition of it, in general commodities, has merely been diverted by the state into payments of wages to functionaries who might have competed in the market for labor and contributed to reduction of wages, into purchase of goods other than the normal consumption in goods of the people or into payment by some means of external debt. Thus, the purchasing power of the German people is curtailed by taxation and the revenue derived from that taxation is partly devoted to the payment of the deficit in the German railways arising chiefly from the employment upon these of men who otherwise would compete in the labor market, draw from unemployment funds or receive military or invalidity pensions. The revenue is also partly devoted to the payment of the salaries of numerous probably unnecessary functionaries employed by the state departments for the same reasons. The revenue is also partly devoted to the payment of the Reparations and the costs of the armies of occupation—in other words, on account of external debt.

In England and France the purchasing power of the people is curtailed for substantially similar reasons. In both countries the government administrations are loaded with functionaries beyond the economical point, but this condition is difficult to alter because of the social reactions to which alteration would give rise. In England taxation is relatively high because of the policy of the government, which from the beginning of the war has been to defray the costs of the war with all speed in order that future generations may not be unduly burdened. As in the case of Germany, the external obligations of the British government necessitate the setting aside of a large part of the national income as principal and interest.

So far as concerns the interest upon government obligations held by its own people, the payment of this interest does not involve a reduction in the national resources; it involves merely distribution of these. It does not even involve redistribution, for this interest is payable in respect to capital which has been furnished to the government by its own citizens largely from the proceeds of their foreign investments, from other savings or from private debts incurred by them in anticipation of future savings. In addition to taxation as the most important element in the restriction of spending power, the necessity of saving in order to repay the funds borrowed for investment in government securities, constitutes an additional restriction, closely corresponding in its effect upon the individual to taxation in the form of a levy upon capital. These considerations apply not only to Great Britain, but also to France and Germany.

(b) In Eastern Europe the purchasing power of the people has been affected by social revolution. In Russia the destruction of the class of landed proprietors and of the class of factory owners and merchants, through the confiscation of their property by the Soviet government or by direct action of the workingmen and the peasants, has destroyed the purchasing power of these classes. Since the Russian peasantry had slender purchasing power owing to the self-contained character of their village life, and since the industries of the towns were disorganized partly through the absence of industrial organizers and partly through the absence of demand for their products owing to the elimination of the classes which consumed them, the purchasing power of Russia as a whole has been destroyed. It can not possibly be recovered until the return of the expatriated consumers or the growth of a new consuming class. Meanwhile, there is no effective demand in the towns owing to the disorganization of industry, and there is no effective demand in the villages owing to the decline of agriculture

due to the destruction of the organized means of disposing of its products.

(c) In America the decline in purchasing power has been due chiefly to the decline in the relative value of agricultural products. Since the United States is predominantly an agricultural country, the number of consuming persons who are engaged in agriculture and its ancillary industries is greater than the number of consuming persons otherwise employed and increased industrial wages do not necessarily imply net total increase of purchasing power on the part of the people as a whole.

From these various causes the demand for commodities has declined as compared with the period before the war, and this decline may fairly be held to be one of the reactions of the war.

Movements of Prices

I have noticed the tendency towards the diminution of the purchasing power of gold; otherwise, the advance of prices from about 1886 up till the summer of 1914. On the outbreak of war, there occurred a general collapse of credit, everything was for sale for ready money and prices fell even for those commodities which obviously must within a very short time be urgently demanded for war purposes. So soon as Great Britain began to organize her industrial life on a war basis, wages advanced rapidly and the prices advanced of all commodities destined for employment directly or indirectly in war. In the hurry and bustle of the campaign little attention was paid to economy. Those nations which remained neutral at the outbreak of war reaped enormous gains. Roumania, Italy and the United States, all of which afterwards became belligerents, as well as Sweden, Norway, Spain and Holland, gained by the advance of prices of the goods which they could supply or transport.

The reasons for the advances in prices at this time were chiefly the following:

1. Urgency of demand in the case, *e. g.*, of war material.

2. Difficulty of procuring supplies of raw material owing to the commandeering of shipping and to the war risks.

3. Difficulty of procuring labor, owing to volunteering and later conscription.

4. With every advance of wages necessitated by the difficulty of procuring labor, there came a tendency to spend these wages and thus to enhance the demand for commodities and there also came a tendency to diminish rather than to increase the number of working hours or the activity of labor. Patriotic appeals had frequently to be made in order to counteract this tendency.

5. Enhanced profits obtained by those who organized war industries. These new and quickly earned profits were often spent as quickly by those who were not accustomed to the possession and judicious control of large

means. These luxurious expenditures tended to increase prices of certain commodities.

All of these causes affected all countries whether belligerent or not. The last-mentioned cause affected especially neutral countries like Spain, for instance. In that country the profits arising from supplying Spanish products to the belligerents, especially after the competition of Italy was eliminated upon her entry into the war, were so great that demand arose for many commodities not previously used in Spain. In 1917-18 the Spanish dealers practically cleared out the fur market and contributed to an advance in the price of furs all over the world.

Efforts were made by means of emergency legislation to counteract these causes for the advance of prices. Food and fuel control boards were established and people were prosecuted for purchasing more than was necessary for their use.

In those cases where the legislation was supported by public opinion the checking of demand probably would, taken by itself, have conduced to maintenance of a relatively low level of price; but the check to supply which the legislation inevitably involved probably more than counteracted its influence in keeping down prices. The British government was forced into the position of organizing supplies. This it did by establishing a purchasing agency in New York by means of which to a large extent it controlled the price of wheat, for example, not only for Great Britain but for the United States as well. This action probably on the whole succeeded in maintaining a more uniform price, although not necessarily at all times a lower price than might have been arrived at in a market without restrictions.

In Germany and Austria food control was established during the war and this control remains, although in a modified form. The terms of peace, which limited the territory of the new state of German Austria, gave to Czechoslovakia a large industrial district and the patronage of the latter new state by the Entente Powers has enabled it to establish itself to advantage. Whether or not the people can develop political and economical sagacity, qualities which their previous absorption in the Austrian Empire did not permit them to exhibit, remains to be seen. For some time after the erection of the new state, the new rulers seemed determined to make it wholly self-contained. They, therefore, refused to trade with their neighbors, either by practically prohibitive tariffs or total prohibition. Thus they placed so high an export duty upon sugar that Austria imported from Java instead of buying it within twenty miles of Vienna, as might have been done otherwise. The Czechoslovakian policy was found to be im-

practicable, and some months ago trading relations were established between the new state and the surrounding countries.

Currency Systems

I have pointed out that in June, 1914, the currency systems of Europe were on a stable foundation. There had been at times during the previous half century inflation of prices as between 1870 and 1873, but there was no currency inflation. Large gold reserves had been accumulated, the credit of the governments of all of the powers was firmly established, chronically bankrupt countries like Turkey being left out of account. Immediately upon the outbreak of war the credit of all governments declined. The expenses of a general war were inevitably to be enormous and the outcome of it uncertain. Governments must come into the money markets as borrowers and therefore the security of previous issues diminished. Prices of the funded debt of all countries declined. In war immediate funds are necessary. The most immediate source of government funds is the printing press, by means of which government promises to pay may be issued rapidly. Where a government already issues notes, increase in the number of these may be effected for a time without attracting attention and therefore without affecting the credit of the government. Where the government has no notes in circulation, as was the case in Great Britain, a government issue may be made, the extent to which it will be received without affecting the credit of the government depending upon the confidence of the people in the stability of the government. Such notes do not usually avail for foreign payments. Germany, France and Great Britain quickly availed themselves of the immediate resources of the printing press. In all of these countries prices were—with, we may allow, at least partial success—controlled; therefore for a time the purchasing power of the new paper money, inadequately secured as it was by gold reserves, was maintained; but after these issues had reached a certain amount the purchasing power not only of the new issues but of all government notes in circulation declined. Not only did the notes of the various governments decline in purchasing power, but the notes of all the banks, even although these were not government banks, declined in purchasing power also, even although the notes of these banks were amply secured by gold reserves in their possession, owing partly to the reluctance of the public to submit to the inconvenience of having to deal with two prices—one in government currency at a low level of purchasing power and the other privately issued currency at a relatively high level. All notes also declined, because it was evidently within the power of the respective governments to place an embargo upon the emission of gold by the banks.

During the war the issue of paper money was kept fairly well under control, through the control over prices, especially in Germany. Yet the value of the paper money of all countries in terms of gold began to shrink before the close of the war. So far as the German mark or the Austrian krone was concerned, there being no trade between the great commercial countries and Central Europe, the external exchange value of the currency of neither of these countries could not be determined. Determination could only be arrived at when foreign transactions began to take place. Anticipation of such transactions began immediately on the close of the war and the first actual transactions showed that the external world had placed a relatively low value upon Central European currency. Meanwhile Germany and Austria alike had had to disburse gold. With every reduction of the reserve, sound theory determined that the proportionate amount of paper money issued on the strength of the reserve should be withdrawn. In grave emergency expediency generally takes the place of sound theory, and this was not done. Within two years after the outbreak of war, notwithstanding commandeering of gold in various forms, the German government was compelled, in order to obtain necessary raw materials, to draw upon its reserves. It happened that, when France paid the indemnity to Germany in 1871-73, a portion of the gold with which a part of the indemnity was paid was purchased from England. This gold had a unique mark, for it was coined in 1871 and the whole of the gold coinage of that year was remitted to Germany on behalf of France. These British sovereigns were placed in the Fortress of Spandau, as a war hoard, and not a single example of the British coinage of that year was to be seen in circulation or in the possession of the banks. In the autumn of 1916 coins of 1871 began to make their appearance in London. They had been paid by Germany to Holland, Sweden, Spain or Switzerland for supplies furnished by these countries, and through these channels had found their way to England.

Germany thus lost her grip on her currency system during the war. She undoubtedly hoped to recover it after the war was over, but the collapse of German government credit has prevented her from doing so up till the present time. In addition to the extraordinary issues of paper currency during the war, by means of which Germany raised a continuously accruing forced loan, Germany also issued numerous formal loans. By the end of the war, the financial resources of the German government were exhausted. It appeared to be impracticable to attempt to raise further loans. The only plan which commended itself to the German government

was to encourage to the fullest extent the resumption of industry and foreign commerce, and by this means to secure a basis upon which an edifice of taxation or a policy of domestic borrowing could be built. Two means were adopted towards this end: By Article 165 of the new constitution of the German Reich, a body was instituted to be known as the Federal Economic Council. This council was affiliated with a federal workmen's council and this again with local workmen's councils. Some writers have seen in this system an institution rivaling that of the political government and destined to replace it in the direction of the national affairs. Excepting for the presence in the Federal Economic Council of employers, the system is not in essence dissimilar to the Soviet or council system of Russia. It appears to me that the view of these writers is exaggerated. The functions of the economic council, as set forth in the Constitution of the Reich, are strictly limited and unless the council usurped the powers of the Reichstag, which it could only do by means of a revolution, there does not appear any likelihood of its actually governing Germany. If it did so, its economic functions would certainly be neglected for the political functions it assumed. Nevertheless, the formation of this economic council is a significant consequence of the war, the defeat and the revolution. It involves at least the possibility of accord between employers and employed and gives the latter a definite place in the councils in which are determined the directions of economic policy. In order to encourage the rehabilitation of industry and commerce, ordinary and so to say negative means, such as protection, were inadequate, and it was necessary to devise positive means. The means that lay most readily to hand were to be found in the normal relations of the German banks to industrial enterprises. Such enterprises had been all along fostered and controlled by the banks to an extent unknown in America. Bank directors devised and organized new enterprises and placed in charge of them persons in whom they had confidence. What was needed was an expansion of this already existing mechanism. But the banks had been obliged to place their resources in the hands of the government, and these resources had been utilized by the government in the prosecution of the war. German private capital had been largely absorbed into government loans. The government was unable or it was presumed to be unable to raise new loans; the banks could obtain from an impoverished people no fresh resources. How could the industries be financed until they began to make returns? The method adopted was in effect for the government to print mark obligations and to deposit these to its credit with the banks. The banks could then furnish the credit they

thought expedient to the industrial and commercial concerns already on their books. If the process could be carried on with due restraint, there was at least a possibility that the profits of industry would eventually justify it. If some such expedient were not adopted, German industry might not recover for a generation, or it might not recover at all. The foreign channels of trade which Germans had opened up in past years would be used by others. Germany would become predominantly an agricultural country, and the comparatively sterile character of most of her land would prevent her from maintaining even her existing population, while growth would be impossible.

Thus the printing press was set to work. A paper money policy is like the slide of a toboggan—progress is slow at the top and it gradually becomes accelerated. If the toboggan meets with an obstruction, or if it diverges from the straight path into the ditch the consequences may be fatal. If, however, the descent becomes less steep and the vehicle glides smoothly on to the plain at the foot of the hill all is well.

A paper money policy may have any of these issues. Like the toboggan it may land its people in the ditch or it may stop of its own accord smoothly and without material damage to any one.

Germany had before her a choice of evils. In any case, her position was a hard one. By that I do not mean that she is entitled to sympathy. She elected to go to war, she sacrificed everything in the course of the war and in the end she was defeated and destitute; but she made others destitute and had indeed seriously diminished the power of her victorious enemies to assist her to rise again, even if they were generous enough to desire to do so. Some German economists object to the financial policy of the government on the ground that it threw upon the mark the whole weight of national reconstruction and that this reconstruction might have been accomplished without imposing a strain on the mark which it could not possibly sustain. None of these critics have advanced, however, so far as I am aware, any practical means which the German government could have adopted to save the mark and to rehabilitate industry. It may turn out that they have not rehabilitated industry permanently, although they have sacrificed the mark. Yet it must be allowed that Germany has been working at the highest pitch which contemporary conditions rendered possible during the past four years. Some other plan might have done the same thing and done it better without any collapse at the end; but no such plan was forthcoming four years ago nor is any such plan really forthcoming now.

Austria did not adopt the same policy as Germany. She

adopted the policy of drift. She paid out her gold and issued paper as required. Her currency system went to pieces as her political system had gone. Yet so paradoxical has life become that in some ways Austria is better off than Germany, for her rural industries are conducted with a minimum of capital, and Vienna is still an important financial center, although every second person who is to be met in the streets of Vienna seems to be dealing in exchange.

Czechoslovakia and Hungary are better off—one because she has industries and financial assistance for them from the Entente, especially from France, and Hungary because she has the rich plains of the valley of the Danube left to her, with fertile farms and enormous herds of cattle. Both of these countries are in the throes not of inflation but of deflation. For deflation has also its difficulties. Rise of paper prices, in consequence of excessive issues of paper, has as one of its results danger of a sudden check. The Hungarian krone was coming back to par too quickly. Its progress had to be checked. So also was the Czechoslovakian krone rising too rapidly. Wages and prices alike were affected, and measures had to be taken to relax the speed of recovery. In these countries, the approach to normal conditions may confidently be anticipated.

The gold situation at the present time may be described as follows: The total amount of gold in the bank and government reserves of the world is approximately twice the amount in the reserves in 1913. Russia has parted with practically all of the huge stock of gold she held at that time. The absence of credit has compelled the Soviet government to send the gold abroad, and the absence of organized production has prevented it from getting any of it back again. Austria has parted with practically all of her gold. Germany has nearly as much gold as she had in 1913. The following countries have greatly increased their gold reserves: Netherlands, France, Switzerland, Spain, Great Britain, Sweden, Norway, Denmark, Roumania, Japan, Australia and the United States.

The normal flow of gold from one country to another has been impeded by legislation adopted during the war and lasting beyond its duration, by the absence of active demand for commodities, by the absence of important transference of capital from one country to another for investment, by the decline of credit owing to the effects of the war and of the peace upon many national finances.

Stabilization of the presently unstable currencies, like those of Germany, must take place at some period, either by means of a legislative act, written in appropriate terms and passed at an ap-

propriate time, or by the mere refusal on the part of everyone to accept the depreciated mark. In the latter event currencies of all kinds will make their appearance in Germany, probably including metallic pieces that have long passed out of circulation. Ancient hoards will be drawn upon and foreign currency will be used extensively. Already a very large amount of one pound notes of the British Treasury, known as Bradbury's, circulate freely in Central Europe and appear in the reserves of bankers. So also do Dutch guilders and, I believe, though to a less extent, United States dollars and Swiss francs.

When the threatened crisis comes in Germany there must be a disturbance of industry and of the export trade, but the essence of the crisis must be in the currency situation.

Wages and Conditions of Labor

Depreciation in the value of gold which we have found expressing itself in the rise of the prices of commodities not merely during the war but for thirty years before the outbreak of war expressed itself also in the rise of nominal wages. There is, of course, no such thing as a universal or immediate rise either in prices or in wages. Advances in either proceed invariably gradually, although the steps may be irregular. An important advance in prices of the necessaries of life must have as consequence either the advance of wages of the whole group of laborers affected or the diminution in the numbers of that group until the wages of the remainder advance because of the scarcity of labor. Wages, however, depend primarily not upon the cost of the necessaries of life, but upon the net return to the product of the labor for which the wages are paid. An author, for example, receives the market value of the product of his pen, which value may not only be below his own estimate of the worth of that product but may be quite inadequate to procure for him the means of life while he is devoting himself to writing. The persons who purchase his writings are not concerned in the least whether he makes a living wage out of them or not or even whether he lives or dies. There is no necessary author, nor is there any necessary laborer or necessary individual man. Yet, if writing did not on the whole yield a reasonable return and if laboring on the whole did not do so, there would be neither writers nor laborers. Advances in wages have not occurred because the prices of the necessaries of life have risen, although the advance of these is often made the ground of a demand for the advance of wages; but they have occurred because the rise in price of the product of the labor in question has enabled the organizer of labor to accede to a demand for increased wages

or to anticipate such a demand. This condition does not depend upon the employing system but must exist in any system which involves the payment of wages for work done—as, for example, municipal or state employment.

Wages have thus risen in those industries in which the price of the product has risen, and the price of the product has risen because of the increased urgency of demand for it. There are always two persons in the making of a bargain, and it is, therefore, impossible for one of these persons to determine the price; but if the seller is urgent, if he must sell, the price which is fixed upon will be near the price which he would take rather than not sell at all, whereas if the buyer is urgent the price will be near that which he would pay rather than forego the purchase. The buyer's price and the seller's price are each determined by the urgency or otherwise of their need.

When the nations were organizing themselves for war, they were extremely urgent buyers—the prices were a secondary consideration. What they wanted were munitions. While the customary scales of prices and wages were upset, the fundamental principles of economic transactions were not otherwise than exemplified.

Many of the second rate newspapers spoke of the laws of political economy being upset by the war, and of the "doctrine of demand and supply" being in abeyance. After that it was only a slight step, which some of them made, to the statement that the war had altered the laws of nature.

These are absurdities. The nations during the war were most urgent buyers, and they had to pay the extraordinary prices that even in time of peace the unusually urgent buyer has to pay. The war did illustrate this fact, however, *viz.*, that what is important is the supply of goods at the place and when they are wanted, not the pecuniary price that is paid for them. Even if debt has to be incurred in respect to that pecuniary price, by the time the debt comes to be paid, who can estimate whether the pressure of that debt will be more than equivalent to the value of the life that is saved by the presence of the goods here and now?

The demand for munitions and the demand for labor to produce them, together with the scarcity of labor in consequence of that demand supervening upon the demand for troops, impelled many of those who had the task of organizing war industries to improve the conditions under which labor was carried on. Many factories were built, for example, specially designed for the labor of women, with comfort of which pre-war factories were destitute. So also in the case of factories in general, more attention was paid to the convenience of the worker than ever before. The demands

of the War Offices were candidly explained to the employes and in regard to many war contracts the profit of the employer was well known. There was no fluctuation of employment. It was a hard and steady grind—day shifts and night shifts alternating. The heads of the enterprises no longer found their time divided between factory management and devising means whereby they might obtain orders to keep their factories working; the orders came to them unasked, they had only to execute them with all speed.

The war thus transformed industry and regularized it. The consequence was unprecedented output. By the end of the war factories in every one of the belligerent countries were producing munitions with a speed which would have been regarded as miraculous at the beginning of the war.

Probably some of this speeding up of industry might have remained after the war, and might under adequate direction have influenced the conditions of the peace; but the transition from war to peace would in any case have interrupted the regularity of the system and would have involved a material change in the motive for such unremitting energy. No pecuniary motive would have sufficed.

After the war, even the munition makers—in all countries, even those which were the least war weary—were rejoiced that it was no longer necessary to work at the top of their speed.

Labor Movements

During the war labor movements in all countries subsided. Internationalist labor propaganda, beginning in 1849, which had some influence in 1862 and again especially in Great Britain in 1884, had not affected the political situation in 1870, nor did it do so in 1914. The French Socialists put France in the first and socialism in the second place, the German Socialists were Germans first and Socialists afterwards. Difference of race, of temperament and of economical conditions made international labor movements practically impossible. Even national labor movements were ignored in the presence of international conflict. So soon as the war was over, the leading socialists were prepared to take advantage of the political situation and to utilize the rich material for agitation offered by the disorders, discontent and miseries which the war had left in the defeated countries and especially in Germany, Austria and Russia. In Russia, the Czarist government was overthrown by a combination of parties of which the Constitutional Democrats were the most influential. The government formed by this combination was succeeded by the Dictatorship of Kerensky, and this was succeeded by the Soviet or Labor government. The Soviet government was composed not of genuine repre-

sentatives of the Proletariat but of Social Democrats who had adhered to the fraction of their group led by Lenin. They immediately declared war against socialists of all other parties as well as against the property owning classes. Hundreds of thousands of these were killed and the government of Lenin established itself. Then began a great experiment. Lenin attempted to build a communist society composed of about a hundred and forty millions of people, the populations of Finland, Poland and the Baltic Provinces having broken away from Soviet Russia. This experiment—by far the largest communist experiment ever attempted—immediately met with great difficulties. The peasantry opposed it either with active hostility or sullen acquiescence in what appeared to be inevitable. The industrial proletariat in the towns melted away into the villages from which they had come. Industry was disorganized partly through the absence of workers and partly through the destruction of the class of industrial organizers. Under these circumstances transportation broke down, exchange of commodities, a necessary postulate of a communist or any other system was at an end. Peasant agriculture declined and famine supervened. The Soviet government changed its policy in 1921 after four years of fruitless effort to establish communism and began to seek the assistance of capitalists and capitalist organizers of industry. Progress toward economic recovery has been slow, but there are signs that it is proceeding. Among the economic reactions of the war this Russian experiment must take an important place in history, alike for its positive effect upon Russia and for its negative effect upon all other countries. The impossibility of establishing communism at a stroke has been fully demonstrated.

The effect of the demonstration upon the labor movement in all countries has been salutary. In every country there were extremists who advocated views similar to those of Lenin, and they have either been converted—like Mussolini in Italy—or have been convinced that "the way of order though it leads through windings is the best."

Among social reactions of the war must be counted the revival of efforts to establish councils or boards by means of which employers and employed might discuss their mutual or their divergent interests. The Whitley Councils in Great Britain and the Federal Economic Council with its subsidiary councils in Germany are examples of a tendency towards recognition of the interest of workingmen in the continuity of production.

The economic bearing of the counter revolution, or anti-labor movement, represented by the Fascisti in Italy and the Technical Emergency Force in Germany—both originating in volunteer groups organized to break strikes and to prevent proletarian domi-

nation—must be recognized. Such movements are a very natural counter-active to aggressive labor movements, and their members may be presumed to have the advantage over the latter of superior education. In Germany the technicians seem to be separating themselves from the Socialist ranks. Since they formed the most energetic and reliable elements in these ranks, their growth may be quite disastrous to the German Socialist groups.

Movements of Capital

The war arrested movement of capital excepting under governmental control. Immediately after the Armistice, movements of capital were resumed. Exact statistics are wanting, but it is known that large amounts of foreign capital have been invested in Germany during the past three years. The investment of this capital has undoubtedly rendered more complicated the question of Reparations; because a capital levy upon these investments, in order to yield funds to pay Reparations, would be looked upon naturally as a breach of faith and would intensify the ill feeling which already exists against Germany abroad. Otherwise, the conditions already described as existing in nearly all countries have militated against the movements of capital from one country to the other. In so far as reparations have been paid by Germany, they have been effected by means of the export of capital, there having been no other fund from which to draw the payments.

Mobility of Property in Land

The pressure of taxation and the advance in wages for service have together conduced to an unprecedented mobility of property in land, especially in Great Britain. Estates have been broken up, country houses have changed hands; those who succeeded in acquiring fortunes during the war have purchased them or they have been converted into schools and other public institutions. Lands of corresponding character with regard to proximity to centers of population are more easily acquired and are acquired at lower prices in Great Britain than in the United States. This condition exists to a certain degree in France and Germany. The payment for purchased land in Central Europe must, however, be made in foreign currency. No one in Germany or in Austria, for example, will sell for payment in the depreciated currency of these countries. There has thus been somewhat less mobility of land in Germany and Austria than might have been expected. Those sales which have been made have involved foreign purchase or purchase by resident Germans who had command of foreign resources.

Public Opinion Regarding the Limits of the Functions of the State

I have noticed the increase of the function of the state in the period immediately before the war. During the war the state assumed numerous functions with which it had not previously been entrusted in modern times. Where railways had been under private ownership, as in Great Britain and the United States, the governments took them over. Where prices had not been controlled, the governments controlled them. Municipal ordinances were passed, greatly extending the area of municipal authority. Not all but nearly all of these emergency functions have since been removed from the state in spite of a certain amount of protest on the part of those who adhere to the policy of collectivism. The practical experience of the exercise of a policy of nationalization and national control seems to have been on the whole adverse to that policy. The chief alleged faults developed during the extension of nationalization were over-manning of the staffs employed by the state, supineness on the part of the state employes, extravagance in time and material and tendency towards arbitrariness in dealing with the public. These objections may or may not have been universally valid; but they had sunk deeply enough into the public mind for little protest to be made when the United States and Great Britain handed back the railways to their proprietors instead of acquiring them for the state. Canada was placed in a different position. She had not taken over the railways; but two of these fell into financial difficulties during the war, and probably to some extent as direct consequence of the war. The government, instead of going to the financial assistance of these railways, purchased them. Up till the present time the management has not succeeded in paying operating expenses. Whether or not in the future this record will be altered remains to be seen.

Public Indebtedness and Taxation

The indebtedness of all countries—belligerent and neutral—has increased as direct consequence of the war. Yet, owing to the fluctuations of exchange and the steady downward tendency of the exchange of Central European currencies, the gold price of the debt of Germany and Austria, for example, has declined. If these countries were now able to purchase their debts with depreciated paper, they might readily reduce them to an insignificant amount. They are not able to do so because some part of this debt is irredeemable and much of this form of debt never comes into the market, and because the moment the governments began to buy on a large scale, the prices of the obligations would rise and the

amounts of paper money which would have to be issued would increase even more rapidly than they have done.

There has been a tendency in all countries to raise the additional taxation required for interest payments and also for increasing administrative expenses by means of a graduated income tax. Beginning in Holland nearly thirty years ago, the graduated or progressive income tax has become practically universal.

Heavy taxes mean redistribution of wealth. The state takes these from the pockets of some people and puts the revenues in the pockets of others. The taxpayer as taxpayer pays taxes, and the holder of government bonds as such receives his dividends. These are often the same person; but the amounts are not necessarily equivalent. Had the governments not adopted the wise policy of issuing public debt in sums of small denomination, thus inducing large numbers to invest their savings in it, the public debt would have been absorbed by a relatively small number of persons and a class of fundholders would have been created. The consequence of alternative action of this kind must have been movements for the repudiation of government indebtedness. There are no such movements in Western Europe or in America.

While the war has thus exercised an influence in numerous economic fields, and while in some of these the influence has been important, it is essential to attribute to the war and to the peace which followed only those reactions which clearly can be traced to them.

SOCIAL LIFE AMONG THE INSECTS

By Professor WILLIAM MORTON WHEELER

BUSSEY INSTITUTION, HARVARD UNIVERSITY

LECTURE VI. THE TERMITES, OR "WHITE ANTS."[1] II

SOME of the most extraordinary idiosyncrasies of the termites are exhibited in the construction of their nests, the termitaria. These bear a certain albeit superficial resemblance to ant nests, but express very clearly the cryptobiotic tendencies of the termites, their great predilection for darkness, moisture and protection from air-currents. Having shut themselves off as much as possible from the outer world and having therefore become almost or quite blind and very thin-skinned, they would be exterminated by predatory insects, birds and mammals and especially by their most inexorable enemies, the ants, if they had not learned to compensate for their feeble and defenseless condition by building unusually strong and resistant nests. There are, to be sure, a few species, like the African *Hodotermes havilandi* and the Indian *Eutermes monoceros*, which are deeply pigmented or have well-developed eyes and forage in the open sunlight, but these are very exceptional.

The nests of termites are of two kinds, the diffuse and the concentrated. The former are not definitely marked off from the environment in the soil or dead wood and consist merely of excavated galleries. Such nests are made by the Protermitidæ and many Mesotermitidæ, *e. g.*, our common *Reticulitermes flavipes*. The concentrated nests are clearly marked off from the environment and are definite, elaborate structures consisting of a royal chamber for the king and queen in the center, surrounded by more or less concentric and inosculating galleries and chambers and an outer covering often of considerable thickness and solidity.

The concentrated nests differ greatly in form in different species. They consist either of earth or woody material or of both, but these substances are subjected to an elaborate preparation. Both the soil and the wood may be swallowed by the workers and after mixture with secretions either regurgitated or passed through the intestine and used as building material, or particles of soil or wood may be merely bitten off and agglutinated with saliva. On drying the substances employed, especially the saliva-impregnated earth, become almost as hard as cement, so that it is by no

[1] Lowell Lectures.

means easy to break into some of the larger earthen termitaria. Termites may build either in or on the ground or on the trunks or branches of trees. When in the former situations the nests are typically conical mounds, but their size and shape may vary greatly. In all cases they differ from ant-nests in lacking exposed entrances. When the termites do not pass to or from the nest beneath the surface of the soil, they construct earthen or carton arcades or galleries, so that they can visit their feeding grounds without exposing their bodies to the light and air.

The most conspicuous type of termitarium in the Old World tropics is a rounded boss or cone built up very gradually over the original subterranean nest established by the royal couple and their first broods of workers. Termitaria of this description are often so large and numerous that at a distance they resemble a village of native huts. Closer study of these nests reveals the existence of several different types which may be conveniently grouped as the more or less conical, the columnar or tower-shaped, the wedge-shaped and the mushroom-shaped. In the open savannahs of Central Africa a large, broad-based, bluntly pointed, grass-covered termitarium is in some places so common as to make large areas of the soil very uneven (Fig. 104). Even larger, conical masonry nests were described and figured by early explorers like Smeathman. The photographs of a more recent traveler, Mr. H. O. Lang, give a better idea of these structures, which may attain a height of 18 to 20 feet (Fig. 105). In Australia the forms of the earthen nests seem to be even more varied. In New South Wales the structures of *Coptotermes lacteus* first appear above ground as broad, flattened, extremely hard bosses, but they gradually grow up till they form smooth tower-like structures 6 to 10 feet high. The largest of these nests are probably about 10 years old. I infer this from the statements of Machon, who found that similar nests in Paraguay reach a height of 12 feet in 11 years. But there are taller nests in Australia. Those of *Eutermes pyriformis* figured by Saville Kent and Froggatt are veritable skyscrapers 18 to 20 feet high. At Koah, in Northern Queensland, I found another type of columnar nest, also the work of a species of Eutermes but short and bulky, often flattened at the summit and narrowed or constricted at the base. A transition between this and a more conical type is represented by certain termitaria found by Saville Kent in the Kimberley district of Western Australia. In the northern part of the same continent we find the singular wedge-shaped structures, called compass or meridional termitaria, because they are constantly oriented with their two long surfaces facing east and west and their narrow ends facing north and south. The pe-

FIG. 104

Mounds of *Termes natalensis* in a grassy plain during the rainy season. Niangara, Belgian Congo. (Photograph by H. G. Lang.)

FIG. 105
Pyramidal termitarium of *Termes* sp. at Kwamouth, Belgian Congo. (Photograph by H. O. Lang.)

culiar mushroom-shaped termitaria are known only from equatorial Africa. They are only a few inches to a foot in height (Figs. 106 and 107).

The tree nests, which seem to be more abundant in the American than in the Old World tropics, are usually subspherical or ellipsoidal and vary from the size of a football to that of a barrel. They are black or dark brown, consist of digested wood and resemble the carton nests of arboreal ants (Crematogaster, Azteca), except that they have a royal chamber in the center and lack the entrance holes in the covering, or envelope. Certain African and Indian tree termitaria are more cylindrical and resemble stalagtites or masses of some viscous substance which has been applied to the treetrunk and has congealed on beginning to drip. In one interesting African species we find on the bark of the tree above a

nest of this type a series of chevron-shaped ridges which seem to be constructed by the termites for the purpose of leading the water that flows down the trunk during heavy shower away from the nest (Fig. 108). In British Guiana Mr. Alfred Emerson called my attention to similarly protected nests constructed by an undescribed species of Hamitermes.

Some termites habitually store food substances in the chambers of their nests. Andrews has shown that the common arboreal *Eutermes pilifrons* of the West Indies keeps its food in the form of large solid, lenticular or conchoidal masses in the center of the nest. Some of the African species of Hodotermes (*H. havilandi*) carry in grass, both green and dead, in pieces about two inches long, and the Indian *Eutermes monoceros* collects and probably stores particles of lichens. The Queensland Eutermes that makes the peculiar bulky nests to which I have referred store their many

FIG. 106

Mushroom-shaped termitarium of *Eutermes fungifaber*. Medja, Belgian Congo. (Photograph by H. O. Lang.)

FIG. 107
Section of same nest as shewn in Fig. 106. (Photograph by H. O. Lang.)

chambers with great quantities of cut grass. Silvestri has encountered plant-storing termites in South America.

Of the many analogies between the ants and termites the most astonishing is that of the fungus-growing habit. Although Koenig and Smeathman, during the latter part of the eighteenth century, independently and almost simultaneously discovered the termite fungus gardens, their true significance has been appreciated only during the past 30 years, as a result of the investigations of Haviland, Holtermann, Sjoestedt, Petch, Doflein, Escherich and Bugnon. While the fungus-growing ants, as stated in a previous lecture, are all confined to a single Myrmicine tribe, the Attiini, and are exclusively American, the fungus-growing termites all belong to a few genera (Termes, Odontotermes, Microtermes, Acanthotermes, Synacanthotermes, Protermes, Sphærotermes) and are confined to the Ethiopian and Indomalayan regions. The fungus-

growers, in fact, represent the most highly specialized members of the order Isoptera and are the ones that make the huge nests of which I have cited several examples. In section the nests are seen to contain a number of large, spherical or subspherical chambers surrounding the royal cell and connected with it and with one another by galleries. In each chamber there are one or more fungus gardens—sponge-like bodies varying in size from that of a walnut to that of a cocoanut, and resembling the gardens of the Attiine ants, but more solidly and more artistically constructed (Fig. 109). They consist of vegetable material which has been collected and comminuted by the workers, passed through their intestines and built up in such a manner as to present the maximum exposure of surface for the growth of the fungi. Petch, who has studied the fungi cultivated by *Odontotermes redemanni* and *obscuriceps* in Ceylon, describes them as growing on the sub-

FIG. 108

Arboreal nest and tunnels of a termite. Niapu, Belgian Congo. (Photograph by H. O. Lang.)

FIG. 109

Mushroom gardens of *Acanthotermes militaris* from a nest at Malela, , Belgian Congo. The white dots are the food bodies. (Photograph by H. O. Lang.)

stratum in the form of a mycelium studded with little clusters of swellings like the food-bodies in the gardens of the Attiini. He has succeeded in growing the fungi in the absence of the termites and finds that they belong to at least two species of mushrooms, *Volvaria eurhiza* (Fig. 110) and *Xyglaria nigripes*. According to Bugnion, the mycelium is sown automatically by the worker termites, since they feed on fungus-infected wood and the conidia pass through their bodies without injury. The fungus-gardens are really the nurseries of the termitarium and are full of just-hatched young, which crop the food-bodies like so many little snow-white sheep. Neither the workers nor the soldiers feed on the fungus, but the king and queen and the other reproductive forms receive the same food as the young.

FIG. 110

Mushroom (*Collybia albuminosa* = *Volvaria* (*Armillaria*) *eurhiza*) growing from an abandoned comb of *Odontotermes* sp. at Coimbatore, India. (Original photograph by W. McRae, reproduced by T. B. Fletcher.)

The nests of some fungus-growing termites are provided with chimney-like structures, communicating with large tubular cavities which have been interpreted by Escherich as a system of ventilating shafts. This interpretation seems to be supported by the existence of somewhat similar arrangements in the nests of the large fungus-growing ants of the genus Atta, and the probability that the successful cultivation of fungi in subterranean chambers depends on a careful regulation of temperature and humidity. Both Petch and Escherich have shown that the diurnal temperature in the termitarium varies only some 9 degrees though the outside temperature may vary as much as 20 degrees or more. On the other hand, the nests of many fungus-growing termites have neither chimneys nor shafts. Trägaordh and Holmgren therefore regard them primarily as passages for the transportation of materials while the nest is being constructed.

Turning now to the association of termites with alien insects we find another striking parallelism with the conditions in the ants described in my last lecture, but again with significant differences. In the tropics termites are very often found nesting in the walls or even in appropriated galleries of nests constructed and inhabited by other species of termites. Holmgren records as many as eight species thus living in the same termitarium. Moreover, many ants, like the species of Carebara, may also live in termitaria. Escherich cites a number of Indian ants as occurring only in such situations, and in Queensland I found fully a dozen species that seemed regularly to inhabit the nests of *Coptotermes lacteus* and allied termites. Certain termites, too, seem to occur only in the nests of certain other termites, *e. g.*, *Anoplotermes fumosus* of Southern Texas and Northern Mexico, according to Snyder, and *Eutermes microsoma*, which has similar habits in South America, according to Holmgren. Silvestri found that the South American *Microtermes fur* always usurps part of the nest and steals the stored food of *Eutermes cyphergaster*. In India Escherich found species of four genera (Leucotermes, Eutermes, Eurytermes and Hamitermes) living in the nests of the fungus-growing species of Odontotermes. With these same termites occur also two species of Microtermes (*globicola* and *obesi*) which form small fungus gardens in their own galleries, though they steal both the fungus and the materials for the substratum from their larger neighbors. In all these cases, whether of termites associated with other termites or of termites associated with ants, we are dealing only with compound nests. The various species always occupy separate galleries and are usually hostile when brought together. No instances of

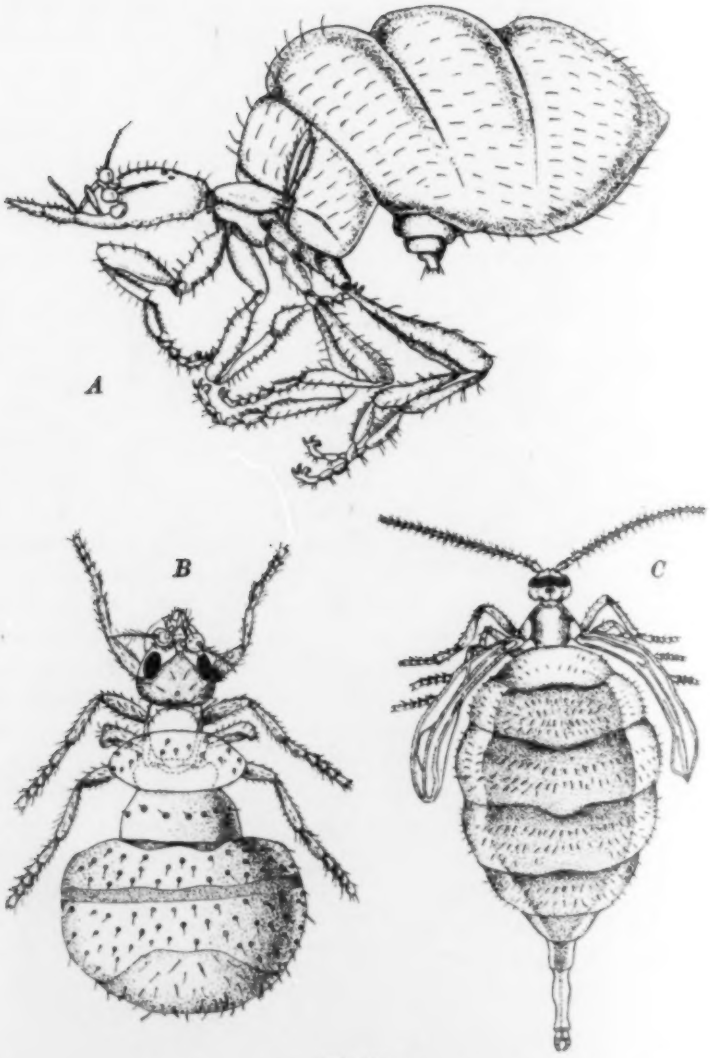


FIG. 111

Physogastric termitophilous flies. *A. Termitoxenia heimi,* a Phorid from the nest of the Indian *Termes obesus.* (After E. Wasmann.) *B. Ptochomyia afra,* a Phorid from the nests of the West African *Ancistrotermes crucifer.* (After F. Silvestri.) *C. Termitomastus leptoproctus,* a Nematoceran fly from the nests of *Anoplotermes reconditus* in Southern Brazil and Argentina. (After F. Silvestri.)

mixed colonies, analogous to those occurring among the ants, have as yet been detected among termites.

More interesting are the relations of the termites to the various insects that live in their nests, the termitophiles, of which several hundred species have been described. They belong to the most diverse orders and families and their association with the termites has evidently been brought about by conditions very similar to those that have induced the myrmecophiles to live with the ants. The termitophiles may also be classified in the same manner, as predators, indifferently tolerated guests (synoeketes), true guests (symphiles) and parasites. Some of the forms seem to have undergone little or no modification as a result of their association with the termites, but many of the true guests and a few of the predators have acquired peculiar characters, the most characteristic of which

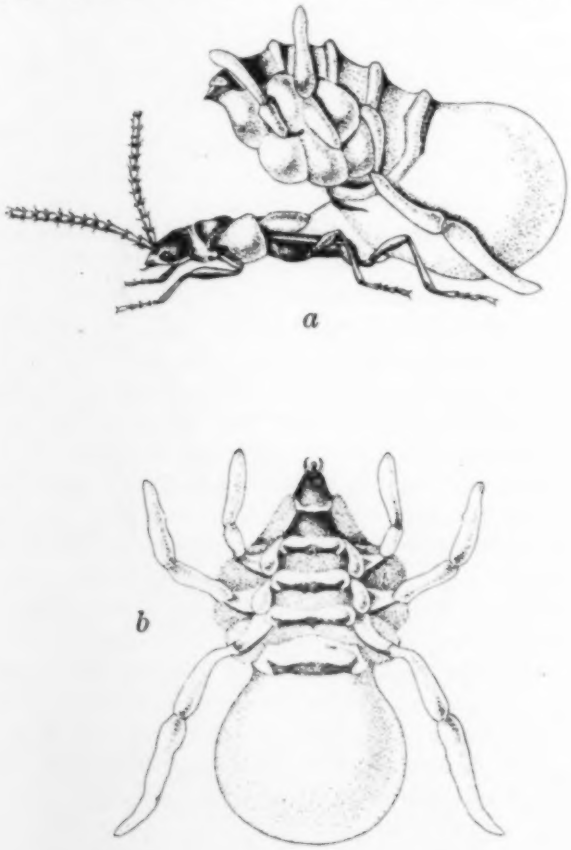


FIG. 112

Spirachtha curymedusa, a termitophilous Staphylinid beetle from South America; *a*, lateral view of the whole insect; *b*, dorsal view of abdomen, showing the three pairs of appendage-like exudatoria. (After J. C. Schiödte.)

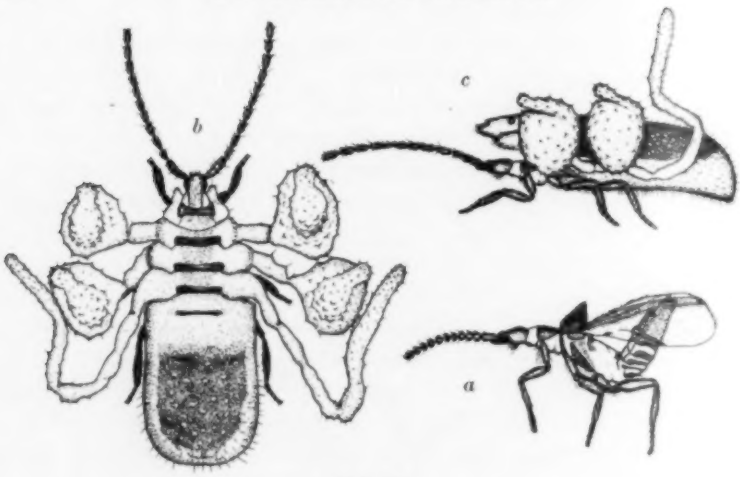


FIG. 113

Termitophilous Staphylinid beetles from the nests of *Nasutitermes* (*Constrictotermes*) *cavifrons* in British Guiana. *a*, *Spirachtha schioedtei* Mann (ms), recently emerged beetle in profile; *b*, *Spirachtha mirabilis* Mann (ms), physogastric form with three pairs of abdominal exudatoria homologous with those of Fig. 112; *c*, same in profile. Probably *a* is the young adult of *b*, though the antennæ differ. (From drawings by Alfred Emerson.)

is physogastry, or excessive enlargement of the abdomen. This may be due to a great increase in the volume of the reproductive organs or of the alimentary canal but in its most typical form it is brought about by hypertrophy of the fat-body. In correlation with the abdominal enlargement there is a decrease in the size of the head and thorax and a reduction or loss of the eyes and wings. In certain species the body may be furnished with segmental, finger-shaped exudate organs, which are occasionally developed even in physogastric forms. I select for illustration a few of the more striking symphiles and predators:

1. A number of small two-winged flies belonging to the family Pheridæ and the genera Termitoxenia (Fig. 111*A*), Termitomyia and Ptochomyia (Fig. 111*B*), which live in the nests of African and Indian termites, show an excessive physogastry, with the accompanying diminution of the head, thorax, eyes and wings. The wings, in fact, are reduced to small strap- or hook-shaped vestiges. Wasmann, who first described the species of Termitoxenia and Termitomyia, is of the opinion that they are hermaphroditic and viviparous, but Silvestri has recently shown that this is certainly not the case with the closely allied Ptochomyia. He has also discovered in the nest of a South American termite (*Anoplotermes reconditus*) another physogastric fly, *Termitomastus leptoproctus* (Fig. 111*C*), belonging to an entirely different fam-

ily (Termitomastidæ). It shows a similar physogastry and reduction in the size of the head and thorax, but the eyes and wings have suffered less diminution.

2. Physogastry of various degrees is also exhibited by many beetles of the family Staphylinidæ and subfamily Aleocharinæ, which are fed and licked by the termites. The most extraordinary forms belong to the genera Corotoca and Spirachtha, established many years ago by Schioedte for three South American termitophiles. In all of them the abdomen is enlarged to form a huge, subspherical mass, which is turned upward and forward over the head and thorax and in Spirachtha bears three pairs of peculiar finger-shaped exudatoria (Fig. 112). When the insect is viewed from above (Fig. 112*b*) only the inverted ventral surface of the abdomen and its appendages are visible. During the summer of 1920 Mr. Alfred Emerson discovered in British Guiana a still more remarkable species of Spirachtha (Fig. 113 *b* and *c*), which has the two anterior pairs of exudatoria greatly swollen at their tips and the posterior pair elongated and lyriform. The strangest fact about this beetle seems to be that when it emerges from the pupa it looks like an ordinary Staphylinid (Fig. 113*a*) but gradually acquires both physogastry and exudatoria during its imaginal life among the termites. Probably this is also the case with other physogastric termitophiles.

3. The larvæ of certain African and Indian beetles of the Carabid genera Glyptus and Orthogonius, which devour the termites among which they live, show a distinct but less pronounced enlargement of the abdomen.

4. The larvæ of several beetles (Staphylinidæ, Cantharidæ), flies (Anthomyidæ) and moths (Tineidæ), which can scarcely be called physogastric, though the abdomen is well-developed, have, like the adult Spirachtha above described, paired finger-shaped and sometimes jointed exudatoria on the abdominal or even on the more anterior segments. Good examples are the larvæ of the Staphylinids *Paracorotoca akermanni*, described by Warren and *Oedoprosoma mirandum* Silvestri, the larva of the Anthomyid *Epiplastocerus mirandus* Silvestri and of the Tineid moths *Plastopolypus integer* and *divisus* described by Trägaordh and Silvestri.

You will observe that the physogastric termitophiles just described resemble their termite hosts and especially the old queens. They also resemble certain ant-larvæ and the queens of Doryline ants. It would seem therefore that all these convergent cases of abdominal enlargement and accumulation of fat, diminution of head and thorax, blindness and aptery must be the results of living in the same peculiar environment. And we should not be far

wrong in maintaining that the factors responsible for such modifications are confinement within narrow galleries and chambers, a very limited supply of oxygen, absence of light and an abundance of carbohydrate food. We have long known that lack of exercise and plenty of food predispose both man and his domestic animals to obesity. That darkness also favors deposition of fat in domestic animals is shown by the experiments of Ottramare, and we have all heard of the fattening of the geese in the cellars of Strassburg and of the fat-bird (Steatornis) that lives in the caves of Trinidad. Absence of oxygen, moreover, as Dewitz, Bohn and Drzwina have demonstrated, inhibits the development of wings in insects. Andrews says that "the respiratory needs of termites must be slight, since the estimated amount of air in a nest (of the Jamaican *Eutermes pilifrons*) weighing 40 pounds or occupying 4 cubic feet was only 9 volumes for each volume of termite." Whether the partial blindness of many physogastric forms is due to absence of oxygen, as Loeb has suggested in the case of the cave animals, or to the absence of light, as has been universally assumed, can not be decided because both conditions obtain in the nests of termites. Of course, the diminution of the eyes and wings may partially account for the reduction in the size of the head and thorax.

Now the obviously degenerative or pathological phenomenon of fatty physogastry and the development of exudatoria enable the termitophiles to become true guests, *i. e.*, they can, like the termites themselves, produce exudates which are eagerly devoured by their hosts and in return either receive regurgitated food or manage to prey on the defenseless brood. Wasmann believes that the physogastry and exudatoria are produced through "amical selection" by the termites, much as fat breeds of pigs and cattle are produced by man's selective activities, but such a hypothesis seems to me even less acceptable than the very similar hypothesis which he advanced to account for the symphilic myrmecophiles.

Of more importance in the lives of the termites than their physogastric guests are the numerous intestinal Infusoria which have been studied by more than a score of investigators since they were first described by Lespes in 1856 and Leidy in 1877. These Infusoria occur as a rule only in the soldier and worker. They have been found in many species of termites in all parts of their range and have been variously interpreted as parasites, commensals and symbionts. Imms, who has published the most recent account of their behavior, regards them as true symbionts, which break down the particles of ingested wood and render them more easily assimilable by the termites. Animals as high in the scale as insects must find it difficult or impossible to digest crude cellu-

lose. It is therefore interesting to observe, as Imms remarks, that the symbiosis between the intestinal Protozoa and the termites is paralleled by the occurrence of numerous genera of Infusoria in the stomachs of ruminants, notably of the ox, sheep, goat, camel and reindeer. "It is believed that, by means of their action upon the vegetable matter consumed by the Ruminants, these Infusoria help to render it capable of being digested by the latter. Furthermore, Infusoria are absent from the stomachs of the young Ruminants prior to being weaned from their parents (*vide* Neveu-Lemaire, 1912, p. 446). According to Certes (1889), glycogen is present in the protoplasm of the Infusoria, and the latter perform a special rôle in the digestive process of the Ruminants. Gruby and Delafond (1843) maintain that the protoplasm of the Infusoria is itself digested, and thereby contributes towards the nutrition of the host Ruminant. Similarly, the Infusoria inhabiting the large intestine of the Equidæ are possibly symbiotic in their relations with their host."

In conclusion I shall have time to dwell on only a few of the many considerations suggested by the singular parallelisms or convergencies between the termites and the ants, such as the development in both of wingless worker and soldier castes, similar nesting and fungus-growing habits, trophallaxis, relations to guests, etc. The duplication of these phenomena in groups so wide asunder that they are placed by systematists at the opposite poles of our classification of insects may be of some interest to the anthropologist, because the study of human cultures reveals the same or very similar institutions and linguistic peculiarities in geographically widely separated peoples. Some anthropologists attribute such similarities to community of origin, while others insist that they are often inventions of independent origin and development. When we reflect that ants and termites have been able, through slow physiological and instinctive processes, independently to evolve such strikingly analogous peculiarities as those I have described, we can scarcely doubt that different human communities, belonging to the same species and endowed with some intelligence, may frequently have hit upon the same inventions.

A less obvious consideration is suggested by the investigations of Holmgren, who finds that as we advance in our study of the termites from the lowest family, the Protermitidæ, to the highest, the Metatermitidæ, we notice a distinct physical, or morphological deterioration in the species and a concomitant improvement of the nervous system. These changes he attributes to advancing social organization. A study of the ants as a group certainly reveals a similar but much feebler tendency of the same kind as we pass

from the more primitive Ponerinæ to the highly specialized Formicinæ and Dolichoderinæ. And it is interesting to note that human society appears to be tending in the same direction. We are becoming accustomed to the thought that our remote posterity will be toothless, hairless and without olfactory organs, toes and possibly other appurtenances, but we console ourselves with the hope that they will have more and better grey matter and a better social behavior than ourselves. We must look forward, however, not only to physical losses in our descendants but to the loss of many of our institutions and customs, for progressive evolution involves not only an acquisition but also a loss of characters. As De Moor, Massart and Vandervelde have pointed out, "all progress must necessarily be attended by degeneration." This explains why the older generation is always scandalized by the young and never remembers that it scandalized its parents.

In termites the amount of degeneration accompanying social evolution is, as stated, much greater than in the ants, and this degeneration seems to have been brought about very largely by an increasing need for protection. With greater elaboration and solidity of nest architecture the termite colonies came to shut themselves off more and more from the outside world, and all the castes, except the winged males and females, lost their eyes and the tough consistency of their integument. They thus came to resemble the mollusks, crustaceans and certain fishes and reptiles which have withdrawn within a heavy protective armor, and have given up participating in the free competitive and cooperative life of their environment. The ants, with the exception of certain subterranean species, have not been inveigled into adopting this passive and timorous mode of life. Hence it is conceivable that some of the more plastic ant-genera may be or may come to be centers of progressive evolution, but there is every reason to believe that the termites have already reached the end of their course. Until recently we might have been tempted to say the same of certain human races like the Chinese and Koreans, who have shut themselves off for long periods from intercourse with other races, but fortunately the human species is still so young that even centuries of such behavior have failed to leave their imprint on the physical organization, and as long as the more resilient mental and social activities alone are affected, there is always hope of a return to the more open cooperative and competitive life which constitutes the basis of progressive evolution.

THE PROGRESS OF SCIENCE

CURRENT COMMENT

By Dr. Edwin E. Slosson
Science Service, Washington

GEOLOGISTS GET AN EXTENSION OF TIME

There was a deadlock between the astronomers and geologists at the beginning of this century. The geologists, having been converted to evolution by Darwin, needed lots of time for the development of the varied forms of life by the gradual process of natural selection, the only process they knew. Then, too, they figured out that it must have taken at least 300,000,000 years to lay the sedimentary rocks and to make the ocean as salt as it is. Man, who is one of the latest settlers on this planet, seems to have been here more than 250,000 years, and the earliest fossils are buried so deep that animal life must have existed some 60,000,000 years at the lowest estimate.

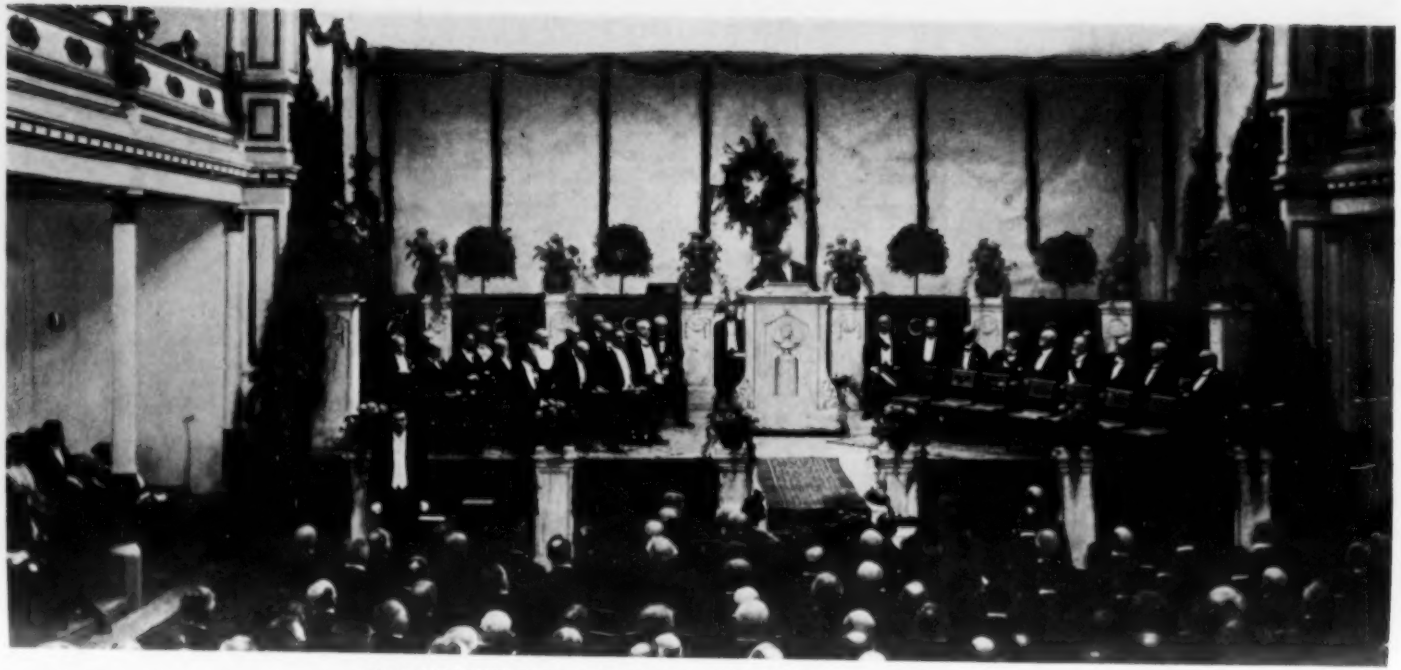
But here the astronomers and physicists interposed a veto on the geologists and paleontologists; "You can't have anything like such a length of time," they said, "for the earth was a molten mass long after the time when you say life began, and was a fiery gas-ball long after you talk of oceans. The earth is the offspring of the sun and the sun itself is only 20,000,000 years old."

This time limit was the estimate of Lord Kelvin, based upon the idea that the sun's heat came from its contraction by gravitation, for no other source of its heat was surmised at that time. He figured out that if all the particles of matter that make up the sun had fallen together from an infinite distance the heat produced by their impact would not be more than enough to keep the sun radiating at its present rate for more than twenty million years.

If, however, somebody should discover another and more abundant source of heat than the shrinkage of the sun, then, of course, the astronomers would be willing to grant the geologists an extension of time for the building up of the world and its inhabitants.

Well, somebody did discover an unknown source of heat abundant enough to satisfy the most extreme demands of the astronomers. This was Madame Curie with her radium, a metal that is continually giving off heat from a secret store within its atom. It appeared then that some of the heavy elements in breaking down into lighter ones, as radium breaks down into lead and helium, give off large amounts of heat for thousands of years. It was later found that atoms of a light element might combine together to form a heavier element and likewise give off heat in immense quantities. For instance, if a pound of hydrogen were to condense to form helium the resulting helium would weigh a little less than a pound, but there would be given off as much heat as would be produced by the burning of 10,000 tons of coal.

Unfortunately no way is known of working this process, so it will not help us out in this winter's coal shortage, but it has helped the astronomers and geologists out of their dilemma. For the astronomers, having now a source of heat sufficient to keep the sun and stars a-going for as long as even they can imagine, can afford to be generous with the geologists in the matter of time. Professor Eddington, of Cambridge, made this concession handsomely when he said recently:



THE CONFERRING OF THE NOBEL PRIZES AT STOCKHOLM

The Nobel Prizes, each of which is of the value of about $40,000, are conferred annually at Stockholm, at which time each recipient is expected to make an address. In the front row to the left are Professor Niels Bohr, of Copenhagen, Professor Francis William Aston, of Cambridge, and Professor Frederick Soddy, of Oxford, who have received the prizes, respectively, for physics in 1921, for chemistry in 1922 and for chemistry in 1921. At the speaker's desk is Professor Henrik Schück, of Upsala University, head of the Nobel Institute.

"Lord Kelvin's estimate of the extent of geological time need not now be taken any more seriously than Archbishop Ussher's and the geologist may claim anything up to ten thousand million years without provoking a murmur from astronomers."

This liberal allowance ought to satisfy the geologists, especially since they have learned from Mendel that evolution may proceed by jumps instead of by the slow accumulation of minute variations which Darwin had in mind.

HIGHWAYS OF KNOWLEDGE

THE first need of a backward country is better communications, roads and railroads, telegraphs and telephones, so the separated settlements of the wilderness can get into touch with each other.

This world is a wilderness, scattered oases of civilization in vast areas of ignorance. Thinkers are few and far apart. Intercommunication of ideas is retarded, often stopped altogether, by the barrier of language as traffic is interfered with by a change of rail gauge at the frontier. Even in the same country minds of different training fail to gear. It does not matter so much if we do not know "how the other half lives," but it is of the highest importance that we know how the other half thinks, especially that smaller fraction of humanity which is thinking for the next century, namely, scientific investigators.

The amount of knowledge accumulated during the last three hundred years since man began the systematic investigation of nature looks large compared with the ignorance that preceded, but looks small when we compare it with what nobody yet knows. What is worse, this precious and painfully acquired knowledge is contained in a few small and perishable packages. I do not mean books, I mean brains. We say "knowledge is power" but the knowledge that lies in libraries has no more power than unmined coal. Can that be called "knowledge" which nobody knows? If all the books in the world were suddenly destroyed how much learning would be left? And how many heads would be holding it?

Professor J. Arthur Thomson, of the University of Aberdeen, puts the point in his usual effective fashion when he says in his book on "The Control of Life": "When we think of the more effective and less wasteful exploration of the earth, or of gathering the harvest of the sea, or of making occupations more wholesome, or beautifying human surroundings, or of exterminating infectious diseases, or raising the health rate, or of improving the physique of the race, or of recognizing the physiological side of education, we are amazed at the non-utilization of valuable—though confessedly incomplete—scientific knowledge. Much has been done, but it must be confessed that man has been slow to follow science in the possession of his kingdom. Part of the reason is that we have not become accustomed, except in some directions, *e. g.*, medical treatment, *to believe in science;* but a great part of the reason is a deficiency of character, that we do not care enough, that we lack resolution."

That is plain speaking and goes to the bottom of the difficulty. It is "deficiency of character, that we do not care enough" to even learn what little has been learned about the management of matter and especially the management of mankind. Science may be discovered by the few, but it has to be applied by the many.

Waste of energy, waste of natural resources, waste of life, waste of time, all forms of waste go back to the waste of ideas. For there is already enough wisdom in the world to make the whole human race more comfortable, healthy and prosperous than any individuals have so far been. But no country is yet thoroughly civilized, even from the stand-

perched to make an address. In the front row to the left are Professor Niels Bohr, of Copenhagen, Professor Francis William Aston, of Cambridge, and Professor Frederick Soddy, of Oxford, who have received the prizes, respectively, for physics in 1921, for chemistry in 1922 and for chemistry in 1921. At the speaker's desk is Professor Henrik Schück, of Upsala University, head of the Nobel Institute.

Wide World Photo

PROFESSOR EMMANUEL DE MARGERIE

Professor of geology in the University of Strasbourg, who is now lecturing in the United States.

point of our present knowledge. To bring that about we must bridge the rivers of ignorance and new highways through the jungles of superstition.

PERCENTAGE ALIVENESS

WHEN I dropped into Professor Winthrop J. V. Osterhout's laboratory at Wood's Hole by the sea I found him at first quite too busy to talk to me. Every minute or two his assistant would hand him a porcelain dish containing a few drops of cell sap which he would hastily analyze by counting the number of drops from a pipette that would change its color. It was a familiar chemical test and in fact I would have thought myself in the laboratory of a chemist instead of a botanist if it had not been for some basins of dank seaweed lying around. There was a lot of electrical apparatus, too, galvanometers, resistance boxes and the like, such as no botanist ever bothered with in the days when I was young.

In those days the boundaries between the sciences were well defined and a professor knew what he was professor of. Even if he taught two or more sciences he was careful not to mix them. A botanist did not have any use for a chemist unless he wanted to borrow paraffin or alcohol from him. Now the botanists and zoologists seem to be going over in a body to chemistry and physics. And it is a question how much will be left of the biological sciences when the physical sciences get what they want out of them.

The differences between the old botany and the new became still more apparent to me when Professor Osterhout explained to me what he was doing and what he was aiming at.

He was endeavoring to apply quantitative measurement to the processes of life, to find mathematical formulas that would show just how much a plant or animal cell is alive or how near it is to the zero point, which we call death. Mathematics is another thing that the old-fashioned botanist had no use for. It is as easy to count the petals on a flower as to count one's fingers. But Professor Osterhout's new book, "Injury, Recovery and Death," is chock-full of mathematical symbols of the toughest sort.

He finds one of the best ways to measure vitality is to determine how well a cell conducts electricity. For the protoplasm that fills all cells offers considerable resistance to the passage of a current so long as it is alive, but as soon as it is dead its resistance falls off. If it is partly dead its resistance is measurably reduced.

Give Professor Osterhout various samples of seaweed of which some are thriving and others have been injured in varying degrees by putting them into water that is too fresh or too salt, or by exposure to hot sunshine or by poisoning with nicotine. They all look equally green and healthy, but by testing the conductivity he can tell you which have been injured the most and how much.

What is more, he can tell which have been injured beyond recovery and which will be restored to a state of normalcy on being put back into ordinary seawater. If, for instance, a strip of eel grass has been injured to the extent of five per cent. by over-salting, it will recover fully when it gets back into its native element. But if it has been injured 25 per cent. its electrical resistance rises to only 90 per cent. of the normal. If the injury amounts to 90 per cent. there is no recovery.

He finds whatever alters the electrical conductivity of plant or animal tissue, whether it be a crushing blow, too much heat, lack of air, lack of water, presence of poisons or anything else, will shorten or impair the life of the organism. He comes to the conclusion that life is a series of balanced chemical processes and that when this balance is disturbed by a change in the environment one process goes faster than another and

Wide World Photo

FATHER AND SON

M. Ratoucheff, the Russian impresario, with his son André, who is twenty years of age. The son, who is twenty-six inches in height, is the leader of the Russian Midgets now playing in London. M. Ratoucheff is six feet four inches.

Wide World Photos

TWINS IN ONE FAMILY

Four sets of twins in a family at Martin's Ferry, Ohio. There are in the family two other children who are not twins.

then the creature grows or decays, thrives or declines. Dying is therefore a normal part of living. The only danger is in its getting to going too fast.

Professor Osterhout does not say anything about the application of his discoveries and theories to human life. So far as I know he has not carried his experiments farther up the scale of life than frog's skin. So it will be some time before we can know whether there is any sense in our crude quantitative expressions of vitality, "I feel half dead" or "more dead than alive," by which we mean usually that we are "dead tired."

THE ANCESTRAL SCANDALS OF SCIENCE

TRACING back the history of a science is like searching out a genealogy; one is sure to unearth something scandalous if he goes back far enough. John G. Saxe warned the would-be ancestor worshipper of this danger in the familiar lines:

> Depend upon it, my snobbish friend,
> Your family thread you can't ascend,
> Without good reason to apprehend
> You may find it waxed at the farther end
> By some plebeian vocation!
> Or, worse than that, your boasted line
> May end in a loop of a stronger twine,
> That plagued some worthy relation.

The chemist handles with reverent awe the latest unearthed and earliest written text of his science, a scrap of Egyptian papyrus, but when he gets it translated he finds it is a counterfeiter's recipe, a method of making base metals look like gold. Or else it is a recipe for cosmetics which is also a form of counterfeiting.

The astronomer finds in a Babylonian brick the first record of the stars but discovers to his disgust that the cuneiform inscription is an astrological treatise, a fortune-teller handbook.

Hero of Alexandria described the turbine steam-engine, the coin-in-a-slot machine and other valuable inventions. But what were they invented for? So the priests of the

temple of Isis could perform fake miracles.

Pythagoras discovered the law of the hypotenuse—and was so happy over it that he killed a hundred oxen. It is hard for us to see why. But mathematics was to him a form of magic, otherwise he would not have been interested in it.

Paracelsus did much to advance medicine. We can not yet dispense with the three drugs he introduced, mercury, opium and antimony. But Paracelsus's real name was Bombast—and he lived up to it.

It is humiliating to confess, but the progress of science in its early days owed much to the false pretensions of its practitioners. Kings would not have kept a corps of men studying the stars unless they had proffered practical returns in the way of auguries. Chemists were subsidized for centuries because they promised the philosopher's stone and the elixir of life—promises not yet fulfilled.

Columbus would not have ventured to cross the Atlantic if he had not been wrong in his figuring about the size of the earth and his royal backers would not have put up the money for the voyage-if he had not told them wrongly that he could reach India that way.

Ponce de Leon was led to Florida by his search for a mythical Fountain of Youth. Coronado explored the Kansas plains to find the fabulous Seven Cities of Cibola. The vain search for the impracticable Northwest Passage to Asia was the stimulus to exploration for a century.

Fortunately for the world, fictitious aims may lead to real results. The scientist has learned how to achieve greater miracles than he ever pretended to perform. Truth has grown up under the shadow of error as infant oaks get their start under the shelter of worthless weeds. In chasing a will-o-the-wisp one may catch sight of a fixed star. Falsity has often served as a guide to Truth.

If the alchemist and the astronomer had been frank with their royal patrons and said: "No, we can not promise you gold from lead, or everlasting life, or the power of reading fate in the stars, but if you will grubstake us and our successors for two thousand years we may be able then to tell you the size of the universe and the structure of the atom," they would have been laughed at instead of getting a share of the king's bounty. Even were they to have added the further promise, "If given a chance to devote our lives to science we will, beside said increase of human knowledge, throw in dynamos, bridges, coal-tar dyes and the like," still the ancient monarchs, being near-sighted, like all men, would have refused to come down with the cash for the benefit of a remote posterity.

But science now-a-days can show such practical profits that it is beginning to get funds for research without pretending to do more than it knows it can. Science can safely promise rich rewards for money spent in its advancement, but it can not say when or in what coin the world will get dividends on such investment in future.

SCIENTIFIC ITEMS

We record with regret the death of Edward E. Barnard, professor of astronomy in the University of Chicago and astronomer of the Yerkes Observatory; of Bernhard Eduard Fernow, professor emeritus of forestry in the University of Toronto; of Wilhelm Konrad Roentgen, professor of physics in the University of Munich, who obtained in 1895 a world-wide reputation by the discovery of the X-rays; of H. G. van de Sande Bakhuyzen, formerly director of the Leyden Observatory, and of Johannes Orth, successor of Virchow at the Berlin Pathological Institute.